Behavioral Responses to Estate Taxation: Evidence from Taiwan*

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Abstract

We quantify behavioral responses to estate taxation by exploiting two large reforms in Taiwan. Using comprehensive administrative data and a difference-in-difference design, we show that the response of reported estates to the reforms is quick, persistent, and exhibits an asymmetry. We estimate elasticities of reported estates with respect to the net-of-tax rate of 2.76 (s.e. 0.39) for the tax increase and 1.31 (s.e. 0.16) for the tax cut. The asymmetry arises because liquid items such as financial assets, deposit savings, and charitable exemptions respond significantly more to a tax increase. The quick adjustments in reported estates, combined with a null effect on labor supply behavior among both decedents and heirs, suggest the responses are likely driven by tax avoidance. The observed asymmetry can be explained by tax avoidance with sunk costs: taxpayers increase avoidance during a tax increase but are less responsive to a tax cut due to previously incurred avoidance costs. We set up a tax avoidance model and derive sufficient statistics, characterized by our estimated elasticities, to assess the welfare impact of tax reforms. Our analysis shows that using the tax cut elasticity, which is attenuated due to sunk costs, would underestimate the welfare cost and overestimate the net welfare effect of a tax increase by 61%.

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1 Introduction

Although extensive research indicates increased wealth concentration (Piketty and Zucman, 2014; Saez and Zucman, 2016; Alvaredo et al., 2017), wealth-transfer taxes are becoming less popular worldwide. Since 2000, ten OECD countries have abolished their estate or inheritance taxes (OECD, 2021). Even in countries like the U.S. that still use them, the exemption threshold has increased twelvefold.[1] This decline in wealth-transfer taxes contrasts with growing concerns that inheritance fuels wealth inequality (The New York Times, 2023; The Guardian, 2024). While some argue that these taxes promote equality, others believe they create distortions and encourage avoidance and evasion, undermining their potential to reduce inequality.

Despite recent progress in theoretical work on optimal estate tax design (Farhi and Werning, 2013; Piketty and Saez, 2013), empirical research on estate taxation remains limited for several reasons. First, detailed microdata on estates are scarce and often truncated, as governments typically collect data only on estates above exemption thresholds. Second, exogenous tax variation among top estate taxpayers is rare, and when available, constructing credible empirical designs is difficult because an individual's estate is only observed upon death, that is, *once* for everyone. Thus, for those who died before a tax change, we cannot observe their post-reform estates; for those who died after the reform, we do not know what their estates would have been absent the reform. This challenge of death being an absorbing state is also encountered in other fields, such as studies on the impact of economic shocks on mortality (Sullivan and von Wachter, 2009; Finkelstein et al., 2024). Given these limitations, earlier studies on estate taxation often rely on state-level and time-series variation (Kopczuk and Slemrod, 2001; Holtz-Eakin and Marples, 2001; Joulfaian, 2006), while recent work uses bunching methods (Escobar et al., 2019; Glogowsky, 2021).

In this paper, we address these empirical challenges by leveraging detailed administrative data and rich policy variation in the Taiwanese context. We are able to link estate tax data to full-population records of wealth and income. Importantly, the estate data consistently include all deceased individuals (henceforth referred to as "decedent"), even those below the exemption threshold over time, thus resolving the truncation issue in previous studies. This consistent coverage is crucial when the tax system changes. To causally identify policy-relevant parameters, we exploit two large estate tax reforms: a tax cut in 2009 that replaced progressive rates of 2% to 50% with a flat rate of 10%, and a tax increase in 2017 that restored a progressive schedule (see Figure 1). These changes provide unprecedentedly large and rich variations in exemption thresholds and tax rates for studying responses to estate tax changes.

To estimate the effect of estate tax changes, we employ a difference-in-difference strategy comparing decedents whose pre-reform estates are above the reform cutoff (treated) to those further below in the estate distribution (control). As noted, a key challenge is that

[1] The nominal exemption threshold for estate taxes in the U.S. was 675 thousand USD in 2001 and 13.61 million USD in 2024, which is around 12 times higher in real terms (Internal Revenue Service, 2024).

for decedents who died after the reform, we do not observe what their estates would have been had they died before the reform, complicating the definition of treatment status. To address this, we leverage the exhaustive panel data on individuals' pre-reform wealth and demographic characteristics to construct their counterfactual absent the reform.

In our baseline estimation, we train a machine learning algorithm on the pre-reform sample to predict each decedent's treatment status, defined by whether their estate percentile falls above the reform cutoff (thus being treated by a tax change) or further below. The algorithm uses various predictors, including total wealth, wealth components, all measured four or more years before death, and demographic variables. The algorithm identifies recent pre-death total wealth as the most informative predictor in determining a decedent's treatment status. Assuming that the relationship between treatment status and predictors would have remained the same in the absence of the reform, we can predict the treatment status of those who died after the reform using their pre-reform variables, which are predetermined and unaffected by the reform. This enables us to assign a predicted treatment status to all decedents in our sample, both before and after the reform.

The validity of our difference-in-difference design relies on two assumptions. The first is the classic parallel trends assumption, which states that treated and control groups would have followed parallel trends absent the reform. The second assumption is that the relationship between treatment status and the algorithm's predictors would have remained the same in the absence of the reform. While these assumptions are not empirically testable, we provide evidence to support their validity. Our event study analysis shows no pretrend in reported estates between the treated and control groups, lending support to the parallel trends assumption. In addition, we show that both the performance and the top influential predictors of the algorithm remain stable across the pre-reform years, suggesting no structural changes in the relationship between lagged wealth components and estates at death that coincide with the reform. We also conduct placebo tests by randomly assigning reform years while applying the same prediction procedures and find no detectable treatment effect.

Leveraging the two reforms that moved in opposite directions, our analysis yields two findings. First, we observe a rapid, strong, and persistent response in reported estates to both reforms. Following the 2017 tax increase, which affected the top 0.5% of decedents, reported estates decreased by 18% after a year and remained around 20% lower three years later. The estimated elasticity of reported estates with respect to the net-of-tax rate is 2.76 (s.e. 0.39). A similarly quick and persistent response was also observed in the 2009 tax cut, which affected the top 4% of decedents. There was an 8% increase in reported estates in the first year after the reform, and the response grew to 20% three years after. The tax cut yields an estimated elasticity of reported estates with respect to the net-of-tax rate of 1.31 (s.e. 0.16). The initial response in reported estates was immediate for both reforms. However, the trajectory of the changes differed: after the tax increase, the decrease in reported estates remained stable over time, whereas following the tax cut, the increase in reported estates grew more gradually in the subsequent years.

Our second finding is the presence of heterogeneity in the *direction* and *type* of tax changes. Among the top 0.5% decedents in both reforms, the elasticity is 2.76 (s.e. 0.39) for the tax increase and 0.47 (s.e. 0.13) for the tax cut, with the former being almost six times higher. Regarding the *type* of tax changes, the 2009 tax cut increased the exemption threshold and reduced tax rates, meaning that some decedents faced a total removal of their tax liability (a repeal), while others had their tax reduced to a non-zero rate (a within-positive decrease). We find that the repeal group has an elasticity of 2.89 (s.e. 0.26), which is greater than that of the groups experiencing within-positive rate decreases. To further support these findings, we complement the analysis with a bunching approach, estimating bunching at exemption-threshold kinks, where individuals move from being untaxed to taxed, and within-positive kinks, where marginal rates change within the positive region. The results confirm that exemption-threshold kinks induce significantly larger responses than within-positive kinks.

In the second part of the paper, we investigate why responses were stronger during the tax increase by examining the anatomy of estate items. Breaking down estates into different asset and deductible items, we find that the asymmetrical responses stem from liquid items like financial assets, deposit savings, and charity and exemption responding more strongly during a tax increase than a tax cut. We further quantify the contributions from assets and deductions in each reform and show that during the 2017 tax increase, 64% of the elasticity of estates is attributed to assets and 36% to deductions. In contrast, during the 2009 tax cut, almost all of the responses can be attributed to deductions.

Are these responses tax avoidance or real changes like labor supply? We explore the channels through which decedents and their heirs adjust. On the decedents' side, we examine the effect of the reforms on their pre-death housing and stock holdings, which are the overlapping assets observed in both the estate and wealth data. We find that the drop in financial assets reported at death following the 2017 tax increase can be partially observed in the decreases in stock before death, especially in privately-listed firms, where many estate taxpayers hold significant ownership stakes and may have influence over the valuation of their shares. In addition, we show the reforms had no effect on labor income, suggesting that decedents did not adjust their labor supply in response to the tax changes.

On the heirs' side, we examine the effects of tax changes on their inheritance and labor supply behavior. The impact on heirs depends on how decedents adjust: *a priori*, if decedents adjust their estates mainly through tax avoidance, the true value heirs receive remains unchanged, and heirs' behavior is likely unaffected. To test this, we identify the decedents' children and track their behavior before and after their parents' death using two sources of variation: (i) the timing and level of inheritance, which is commonly used in the past literature, and (ii) the change in inheritance induced by the estate tax reforms, a source of quasi-experimental variation that has not been previously explored.

Using variation in timing and level of inheritance, we observe around 2% decrease in labor supply on the extensive margin after the decedent's death, with no effect on the intensive margin. This aligns with the past literature that reports a reduction in labor supply

on the extensive margin, with less conclusive results on the intensive margin. However, when leveraging variation induced by the tax reforms, we find no difference between those whose parents died before and after the tax changes. The lack of adjustment in their labor supply suggests that the true value of inherited estates remained unchanged.

These findings, along with the quick changes in the reported estate that responded within a year after the reforms, suggest that the decedents' responses we capture are likely driven by tax avoidance rather than real changes. The asymmetrical responses between the tax increase and the tax cut also support this interpretation. As individuals engage in tax planning during their lifetime, they incur avoidance costs such as setting up offshore accounts, establishing trust funds, or paying professional tax planners. When taxes increase, they may intensify their avoidance. However, when taxes decrease, they may not reduce their avoidance by as much since they have already invested in these avoidance instruments, and reversing prior decisions can incur additional fees.

In the last part of the paper, we use our empirical results to conduct welfare evaluation in the context of a tax avoidance model based on Chetty (2009). In this framework, an individual with initial wealth decides on consumption and estate sheltering, which incurs a sheltering cost. The model can be extended into a two-period framework that rationalizes the asymmetrical responses between a tax increase and a tax cut: in the first period, individuals make decisions as in the static framework. In the second period, if the tax rate changes, individuals can adjust their sheltering, but the marginal cost of reducing previously sheltered assets in the first period is higher than that of increasing them, due to potential penalties or fees such as dissolving trust funds or repatriating offshore wealth. As a result, the model predicts a stronger response to tax increases than tax cuts.

Based on the framework, the welfare effect is characterized by the elasticity of reported estates, as informed by our reduced-form estimates. Our empirical findings show that responses to tax increases and cuts differ both in magnitude and in timing: during the 2017 tax increase, where no sunk costs were present, the response was immediate and stable over time, yielding an elasticity estimate that accurately reflects the welfare effect of the tax increase. In contrast, the 2009 tax cut, introduced when taxpayers had already incurred sunk costs from prior tax planning, showed an immediate response that gradually increased over time as taxpayers adjusted incrementally, resulting in an attenuated elasticity.

This distinction has important welfare implications. For a tax increase, using the elasticity estimated from the 2017 tax increase (without sunk costs) will provide an accurate assessment of the welfare effect. However, if the government mistakenly applies the elasticity estimated from the 2009 tax cut (with sunk costs) to evaluate the welfare effect of a tax increase, it will overestimate the welfare effect by 61% due to underestimating the welfare cost. For a tax cut, both short-run (with sunk costs) and long-run (without sunk costs) elasticities are relevant, as the cumulative welfare impact depends on how quickly individuals recover from these sunk costs.

In summary, the observed asymmetry in behavioral responses to estate taxation has nontrivial welfare implications and may extend to other taxes, such as corporate income

taxation or wealth taxation, where tax avoidance involves similar fixed costs. For example, setting up offshore operations in tax havens often requires fixed costs, which could result in a comparable asymmetry in response.

Related Literature and Contributions. Our paper makes several contributions to the literature on behavioral responses to estate and inheritance taxation, which can be divided into two waves: an earlier wave in the early 2000s that used state-level or time-series variation on U.S. data (Holtz-Eakin and Marples, 2001; Kopczuk and Slemrod, 2001; Joulfaian, 2006), and a more recent wave using European microdata and modern empirical methods such as bunching and difference-in-difference approaches (Goupille-Lebret and Infante, 2018; Mas-Montserrat, 2019; Escobar et al., 2019; Glogowsky, 2021).

First, we leverage exceptionally large and rich tax variation. The two reforms allow us to study the heterogeneity in responses across types (repeal vs. within-positive rate changes) and directions (increase vs. decrease) of tax changes. To our knowledge, we are the first to distinguish between an increase and a decrease in estate tax rates and document the resulting asymmetrical responses in the estate and wealth taxation literature. While asymmetry between tax increases and cuts has been observed in the context of value-added taxes (Benzarti et al., 2020, 2024; Benzarti, 2024), the context of estate taxes presents a different scenario. We attribute the asymmetry to sunk costs in tax avoidance strategies: once individuals invest in tax planning that incurs avoidance costs, it is difficult to reverse when tax rates fall.

Second, we propose a novel methodological approach by developing a difference-in-difference design suited to the context of estate data, where having only a single observation of each individual's estate complicates the definition of treatment intensity. This allows us to go beyond prior studies that rely on bunching (Goupille-Lebret and Infante, 2018; Escobar, 2017; Glogowsky, 2021) or regression discontinuity designs (Escobar et al., 2019), which may only capture local effects, particularly when adjustment frictions or the lumpy nature of estates are present (Kleven, 2021). Similar prediction methods have been applied in other contexts. For example, Jakobsen et al. (2024) apply them to predict wealth following the repeal of the Swedish wealth tax. Building on these techniques, we leverage detailed wealth and demographic data to construct counterfactual estates in the absence of tax changes, enabling us to capture a broader effect of estate responses to tax changes.

Third, the combination of large reforms and detailed data allows us to go beyond estimating the aggregate response of estates by conducting an in-depth decomposition and estimating the elasticity of each asset type separately. In addition, by comparing asset values during a person's lifetime with values reported at death for overlapping items, we capture a fuller picture of tax avoidance adjustments. These findings have significant implications for tax policy design targeting the very wealthy. Identifying which assets are most adjusted in response to tax changes helps policymakers pinpoint areas where tax avoidance is prevalent and underscores the need for stronger enforcement measures, such as enhanced third-party reporting and international collaboration to track offshore wealth.

Our paper also extends the literature on the effects of inheritance receipts on heirs' labor supply, often referred to as the Carnegie effect (Holtz-Eakin et al., 1993; Elinder et al., 2012; Bø et al., 2019; Nekoei and Seim, 2023). Prior studies usually rely on variation in the timing and level of inheritance, and find that receiving an inheritance reduces labor supply on the extensive margin, with mixed evidence on the intensive margin. We contribute to this literature by exploiting a novel source of variation: changes in inheritance induced by estate tax reforms. Unlike the timing of a parental death, which heirs might expect and prepare for, quasi-experimental tax-induced changes in inheritance reduce the potential for anticipatory behavior. Comparing heirs whose parents died before and after the tax changes, we find no significant effect on heirs' labor supply, despite substantial differences in the inheritances received due to the reforms. We interpret our results as evidence that the observed changes in estates are driven by tax avoidance rather than real behaviors.

The rest of this paper is organized as follows. Section 2 describes the institutional context of estate taxes in Taiwan and the data. Section 3 presents the empirical strategies and results of the effect of estate taxation on decedents. Section 4 investigates the mechanism behind the responses. In Section 5, we build a model that connects our empirical findings to aggregate welfare impacts. Section 6 concludes.

2 Institutional Context & Data

2.1 Institutional Context

Estate taxation has a long history in Taiwan that dates back to the 1940s and has undergone several reforms over the years. This section describes the tax schedule during the period covered by our data, from 2004 to 2019. Like in other developed countries, Taiwan's estate taxes primarily affect individuals at the top of the wealth distribution. Approximately 4% of annual deaths in Taiwan are subject to an estate tax. This number is similar to the U.K.'s 4% and higher than the U.S.'s 0.1%. Estate tax revenues account for around 1% of Taiwan's total tax revenues, which is higher than the 0.5% average in other developed countries (OECD, 2021).

Tax Base. The Taiwanese estate taxation is levied on the amount of the estate left by a decedent. This system is also adopted by the U.K., the U.S., and South Korea, and contrasts with the inheritance tax system used in most European countries, where the tax is levied on the amount inherited by an heir.

An estate is defined as assets minus deductions. Assets include housing, financial assets, and deposit savings. For Taiwanese residents, the estate tax covers worldwide assets. For non-residents, it applies only to assets located in Taiwan.

Housing assets include land and houses. Financial assets include publicly-listed and privately-listed stock, bonds, futures, trusts, insurance, and other financial instruments. Deposit savings are savings in banks.

Deductions include charity/exemptions and other deductibles. Charitable contribution-

s/exemptions include donations to nonprofit organizations, spousal exemption claimed by the living spouse according to the Civil Law, and estate taxes paid within the last five years. Other deductibles include debts, farm, funeral, and demographic deductions for dependents, parents, or disabilities.

Assessment and Enforcement. Housing values are determined by prices announced by the government, which are typically adjusted every two years and serve as the basis for property taxes. These prices are generally lower than market prices. Ownership of these assets is third-party-reported and can be verified from the property tax records.

Financial assets include publicly- and privately-listed stock, and other self-reported assets. The ownership and quantities of publicly- and privately-listed stock are third-party-reported, as they can be verified from the dividend tax records. Publicly-listed stock is valued at the market price on the date of death, while privately-listed stock is assessed based on the company's net assets on the date of death. Privately-listed stock is more prone to misreporting, as companies do not usually keep daily balance sheets, and many estate taxpayers closely hold these firms to the extent that they have the power to adjust their valuation. Aside from stock, this category also includes self-reported assets like trusts, insurance, farm output, credits, cash, jewelry, and offshore assets. The government has limited power to detect offshore wealth.

Deposit savings are self-reported but can be cross-verified through third-party-reported interest income information. Individuals must provide relevant bank statements when filing. Appendix Table A.1 presents the details.

There are two types of penalties for underreporting. For amounts owed under 35K TWD (approximately 1,200 USD), the individual must pay the tax with interest but incurs no fine. For amounts exceeding 35K TWD, fines range from one to two times the tax owed. In cases of tax fraud, penalties can include fines of one to three times the owed tax and criminal charges may be pursued.

Estate Tax Schedules. Figure 1 illustrates the estate tax schedule in Taiwan between 2004 and 2019. Between 2004 and 2005, it was a progressive schedule where the MTRs ranged between 2% and 50%. The exemption threshold was 7 million TWD (around 230K USD). In 2006, the exemption threshold was adjusted for inflation, increasing to 7.79 million TWD, while the MTRs remained the same.

In October 2008, the government announced significant changes to take effect on January 23rd, 2009. First, the exemption threshold was raised by approximately 50%, from 7.79 million TWD to 12 million TWD (equivalent to around 260K USD to 400K USD). Second, the progressive tax rates that were previously between 11% and 50% were replaced with a flat rate of 10%. The announcement was rather unanticipated since the government only took office in May 2008, and the estate tax reform was not part of the political discourse during the election.

The reform aimed to achieve several objectives. First, the tax cut was expected to attract wealthy individuals to repatriate offshore investments. It was reported that on average 1.2

trillion TWD (40 billion USD) of funds was sent offshore per year before 2008. The government believed that the reform could stop the funds from going overseas, especially during the unstable financial outlook of the time. Second, the lower tax rate was intended to discourage tax planning. Third, the higher exemption threshold was designed to benefit small and medium-sized family businesses in terms of succession planning. However, the significant tax cut sparked protests from the main opposition party, the Democratic Progressive Party (DPP), at the time, as they claimed it would exacerbate inequality.

In the 2016 presidential election, the DPP candidate proposed raising estate taxes to reduce inequality and fund the Long-Term Care Services Act 2.0. After winning the election, the government introduced two new MTRs of 15% and 20% for the top 0.5% of estates, effective May 17th, 2017.

[Figure 1 here]

Gift Tax Schedules. Taiwan also imposes a gift tax, which is levied on the annual amount of gifts made by a donor. The gift tax schedule closely follows the estate tax schedule.[2] Appendix Figure A.1 shows the gift tax schedule over time. Between 2004 and 2005, the gift tax had a progressive schedule with an exemption threshold set at 1 million TWD (approximately 30K USD). In 2006, the exemption threshold was adjusted for inflation, increasing to 1.11 million TWD, while the MTRs remained unchanged. In 2009, the exemption threshold was raised to 2.2 million TWD, and the progressive tax rates were replaced by a flat 10% rate. In 2017, the flat rate was reverted to a progressive schedule, where two new MTRs of 15% and 20% were introduced.

To deter donors from using gifting to avoid estate taxes, Taiwan regulates that gifts given within two years before death are included in the estate tax base. The values of these gifts are reassessed based on the timing of death. If a gift tax was previously paid on these gifts, the amount can be deducted from the estate tax liability. However, if there is a difference in the values of the gifts between the time of the gift and the time of death, the donor must pay the difference in their estate tax liability.

Our paper examines the effect of these reforms on the estate tax base, ensuring that near-death gifts are included to avoid potential tax leakage. This setting provides two advantages. First, it is policy relevant to countries that regulate near-death gifting such as the U.S. and the U.K.[3] Second, the two-year rule ensures that near-death gifts are included, so the effects we capture would not be dominated by the channel of gift leakages but represent meaningful measures that have important policy implications.

2.2 Data

We utilize the following datasets provided by the Fiscal Information Agency in Taiwan. While Taiwan does not have a wealth tax, wealth information could be derived from other

[2] In the U.S., the estate and gift tax schedules are also tightly linked and subject to the same rate.

[3] In the U.S., the gross estate contains gifts made within three years of death (I.R.C. §2035(a)(1)). In the U.K., gifts given within seven years of death are counted towards the estate tax base, although differential tax rates would be applied depending on the timing (I.T.A. 1984 §113A).

tax sources. The overlapping assets observed both at death (from estate data) and during one's lifetime (from property and dividend income tax data) are housing, publicly-listed, and privately-listed stock.

Estate Tax (2004-2019). The estate data contains all estate tax records filed to the government. It contains the individual's ID, date of death, cause of death, reported estates, assets, and deductions. In the subfiles of the records, assets are stored by housing, financial assets, and deposit savings.

Property Tax (2004-2019). The property tax dataset provides information on land and housing. The land data is collected for land taxes, and the housing data is collected for house taxes. Both datasets contain individual IDs, property IDs, property area, location, and government-assessed values.

Dividend Income Tax (2004-2019). The dividend income tax data records ownership of shares in publicly and privately listed firms. It includes individual IDs, firm IDs, and the number of shares held.

Individual Income Tax (2004-2021). The income tax records include information on various income types, such as interest, dividend, and labor income. It contains individual IDs, the type of income, values, and the firm from which the income was earned, where applicable.

Wealth Construction. We define wealth as the sum of housing, stock, and capitalized deposit savings. When constructing these values, we follow the estate assessment rule. Housing values are based on government-assessed prices. Publicly-listed stock is valued at its market price, while privately-listed stock is valued based on the net assets reported on the company's balance sheet from corporate income tax data. For deposit savings, we capitalize interest income following the method described in Saez and Zucman (2016).

Sample Construction. To construct our sample, we start by extracting individuals who died above the age of 30 between 2004 and 2019. We then merge their estate records and lifetime information from the property, dividend, and income tax data.

2.3 Graphical Evidence on Attentiveness to Exemption Thresholds

To illustrate that individuals pay attention to the exemption threshold and behaviorally respond, we present motivating graphical evidence on the dynamic bunching around the exemption thresholds.

In Figure 2, we bin individuals into 200K TWD intervals and trace the distribution of estates around the exemption thresholds over time. Panel A presents the distribution before 2006, where there was an excess mass at the old exemption threshold (7 million TWD), denoted by the grey dashed line. In 2006, the exemption threshold was adjusted by 10% to account for inflation, moving from the grey dash line to the red dash line (7.79 million TWD). As shown in Panel B, within a year, the excess mass at the old kink disappears and an excess mass appears at the new exemption threshold. The response is persistent and

strong through the end of 2008.

[Figure 2 here]

A similar pattern happened when the 2009 reform increased the exemption threshold substantially by 150% (to 12 million TWD). In Panel C, the excess mass shifted from the original threshold (the grey dashed line) to the new threshold (the red dashed line) within a year. The bunching at the new threshold becomes clearer and stronger over time. To highlight the persistence of the response, Panel D shows the distribution for all subsequent years. The frequencies increase over time as the population grows, and the excess mass formed in response to the 2009 reform remains evident until the last year of our data. This indicates that individuals are highly attentive to the threshold and adjust their estate reporting accordingly.

3 Empirical Analysis

3.1 Difference-in-difference Approach

Goal and Challenges. Our goal is to causally estimate the effect of estate tax changes on reported estates by using the two reforms as sources of variation and setting up a difference-in-difference design comparing those whose pre-reform estates are above the reform cutoff (treated) and those below (control). Define $\text{EstatePercentile}_{t(i)}(\tau)$ the estate reported by decedent i who dies in year t under the tax schedule τ. τ is a vector of parameters that defines the tax schedule and can be the pre-reform tax schedule τ_0 or the post-reform τ_1. Treatment status is defined as $\text{Treated}_i = \mathbb{1}[\text{EstatePercentile}_{t(i)}(\tau_{\mathbf{0}}) \geq C]$ where C is the reform cut-off.

For individuals who died *before* the reform, we directly observe $\text{EstatePercentile}_{t(i)}(\tau_0)$ from the data, making it straightforward to assign them a treatment status. However, for individuals who died *after* the reform, we face a challenge. We only observe their estates under the post-reform tax schedule, $\text{EstatePercentile}_{t(i)}(\tau_1)$. This creates a problem because using $\text{EstatePercentile}_{t(i)}(\tau_1)$ to define the treatment status would create endogeneity, as the estates under the new system may already reflect behavioral adjustments in response to the reform.

Proposed Prediction Solution: The Idea. To address the above challenge, we propose a method to predict each individual's treatment status, specifically whether their estate percentile under the pre-reform tax schedule would have been above the reform cutoff, by leveraging panel data on individuals' pre-death wealth, income, and demographics information.

Using only those who died *before* the reform, we model the relationship between treatment status and their pre-death information as follows:

$$\mathbb{1}[\text{EstatePercentile}_{t(i)}(\tau_0) \geq C] = f(\mathbf{W_{t(i)-k}}, \mathbf{W_{t(i)-k-1}}, ..., \mathbf{X_i}) + \epsilon$$

where $f(.)$ is a function modeling one's treatment status to a set of predictors. The vector $\mathbf{W_{t(i)-k}}$ is a vector of wealth variables measured k years before i's death year t. k is large enough to ensure that for post-reform decedents, $\mathbf{W_{t(i)-k}}$ is predetermined and unaffected by the reform. $\mathbf{X_i}$ is a vector of demographic variables. ϵ is a prediction error.

For all decedents, regardless of dying before or after the reform, we observe their $\mathbf{W_{t(i)-k}}, \mathbf{W_{t(i)-k-1}}, ..., \mathbf{X_i}$, which are all predetermined before the reform and not influenced by the reform. We can therefore apply the estimated model to predict a treatment status for each individual, indicating whether their estate percentile under the pre-reform schedule would have been above the cutoff. In other words, the predicted treatment status is $\widehat{\text{Treated}}_i = \hat{f}(\mathbf{W_{t(i)-k}}, \mathbf{W_{t(i)-k-1}}, ..., \mathbf{X_i})$.

Once the predicted treatment status is constructed, we can estimate a simple difference-in-difference regression:

$$y_i = \alpha_0 + \alpha_1 \widehat{\text{Treated}}_i \times \text{Post}_i + \alpha_2 \widehat{\text{Treated}}_i + \alpha_3 \text{Post}_i + u_i$$

where y_i is the realized estate of decedent i. $\widehat{\text{Treated}}_i$ is the predicted treatment status of i. $Post_i$ is 1 if i dies after the reform.

Proposed Prediction Solution: The Implementation. We outline the implementation using the 2017 reform as an example below, though the same approach applies to the 2009 reform. In the previous paragraphs, we described the prediction concept in a context where the classification of treatment status is binary for simplicity. In practice, we classify decedents into multiclasses based on their estate percentile: (i) under 90th percentile; (ii) between the 90th and 96th percentiles; (iii) between the 96th and 99.5th percentiles, and (iv) above the 99.5 percentile.

Next, we train a random forest algorithm on the pre-reform decedents who died between 2014 and 2016. In other words, the $f(.)$ function described previously is a random forest algorithm here. This training step establishes the relationship between which one of the classes a decedent falls in and a set of predictors, such as total values of wealth and different wealth component values, all measured four to ten years before death, and demographic variables.[4] Appendix B.1 provides further details on the prediction procedures. The algorithm identifies recent years' total wealth values, as the most influential predictors (see Appendix Figure B.2), and model performance is presented in Appendix Table B.2.

Then, we apply the trained algorithm to all decedents who died before and after the reform, i.e., between 2014 and 2019. As we observe all decedents' predictors, which are all predetermined and unaffected by the reform, the algorithm assigns each decedent a predicted class. This predicted class indicates which class a decedent's estate percentile would be in if they died under the pre-reform tax system.

[4]We pick years more than four years far back to ensure that when we predict the estate percentiles of those who died after the reform, all predictors predetermined and unaffected by the reform. For example, for individuals who died in 2019 (the latest year in the study), all predictors are measured four to ten years before death, which would be years before 2015.

Event Study Specification. We allocate decedents into two-quarter bins (180 days) based on their death dates and estimate the following event-study specification:

$$\begin{aligned} y_i = \alpha_0 + \sum_{k \neq \text{BasePeriod}} \alpha_{1k} \cdot \widehat{\text{Treated}}_i \times \mathbb{1}[\text{DeathPeriod}_i = k] \\ + \alpha_2 \cdot \widehat{\text{Treated}}_i + \sum_{k \neq \text{BasePeriod}} \alpha_3 \cdot \mathbb{1}[\text{DeathPeriod}_i = k] + u_i \end{aligned} \tag{1}$$

where y_i represents the estate outcome of decedent i. Given that the 2017 tax reform affected the top 0.5%, we define decedents as treated if they are predicted to be in the class above the 99.5 percentile and as control if they are predicted to be in the class between the 90th and 96th percentiles. Similarly, for the 2009 reform affecting the top 4%, we define the treated as those predicted to be in the class above the 96th percentile, with the control group defined in the same way as the 2017 reform. DeathPeriod_i represents the death period of decedent i, grouped into 180-day intervals. The base period, denoted as 'BasePeriod', is defined as the 180 days immediately before each reform.

The identifying assumptions are as follows. The first assumption is the classic parallel trend assumption in a diff-in-diff design. That is, in the absence of reform, the trends of the outcomes of the treated and control would have been parallel. A threat to this is anticipatory behavior; if individuals anticipated the tax changes and adjusted their behavior accordingly, it could bias our estimates. To alleviate the concern, we perform robustness checks in Section 3.5 by assuming the reform happened a year earlier in both reforms and show they yield similar results. Furthermore, as shown in the event study graphs in the later section, there is no pretrend in the reported estates between the control and treated groups, supporting the validity of the parallel trends assumption.

The second assumption, specific to our prediction algorithm, is that the performance of the algorithm is the same before and after the reform in the absence of reform. Although this assumption is not directly testable, we provide supporting evidence in Section 3.5 by demonstrating that both the algorithm's performance and the top important predictors remain stable across the pre-reform years. This ensures there is no structural break in the prediction model. In addition, we conduct placebo tests by randomly assigning reform years and applying the same prediction procedures to the samples and detect no treatment effect.

Elasticity Identification. We would like to know the elasticity of the reported estates with respect to the net-of-tax rate, i.e., when there is a 1% change in the net-of-tax rate, how much the reported estate would change in %.

$$\begin{aligned} \varepsilon &= \frac{\%\text{Change in Reported Estate}}{\%\text{Change in Net-of-tax Rate}} \\ &= \frac{\mathbb{E}[\Delta \log \text{Estate} | \text{Treated} = 1] - \mathbb{E}[\Delta \log \text{Estate} | \text{Treated} = 0]}{\mathbb{E}[\Delta \log(1-\tau) | \text{Treated} = 1] - \mathbb{E}[\Delta \log(1-\tau) | \text{Treated} = 0]} \end{aligned}$$

The elasticity is the effect of the reform on reported estate (reduced-form) scaled by the effect of the reform on tax rate change (first-stage). In practice, we proxy the treated dummy with the predicted one and estimate the following:

$$\text{First stage:} \quad \log\left(1-\tau_i^{\text{Post}}\right) - \log\left(1-\tau_i^{\text{Pre}}\right) = \beta_0 + \beta_1 \widehat{\text{Treated}}_i + v_i \tag{2}$$

$$\text{Reduced form:} \quad \log \text{Estate}_i = \alpha_0 + \alpha_1 \widehat{\text{Treated}}_i \times \text{Post}_i + \alpha_2 \widehat{\text{Treated}}_i + \alpha_3 \text{Post}_i + u_i \tag{3}$$

In our baseline estimation of the first stage, we use those who died before the reform and construct the mechanical marginal tax rate change on the left-hand side by assigning the old and new marginal tax rates based on their realized reported estates. The coefficient β_1 would imply how much more the treated are exposed to the tax rate changes in the absence of the behavioral effect. In the reduced form, we use decedents from both periods to estimate the treatment effect of the reform on the reported estates. Then, we take the ratio of the two stages to arrive at the implied elasticity. Under the assumption that the prediction error of the algorithm is the same pre- and post-reform absent the reform as stated previously, the prediction errors in the predicted treatment status in the two stages offset each other, yielding an unbiased estimated elasticity.

Using pre-reform decedents to estimate the first stage offers the following advantages. First, it avoids behavioral adjustments. Decedents in the post-reform period may adjust their behavior in response to the reform, potentially underestimating the true mechanical tax rate change. Second, as we later augment the diff-in-diff approach with a bunching approach, using pre-reform decedents to estimate the first stage enhances the comparability between the two approaches. In the bunching method, the tax rate difference is the mechanical difference derived from the distance between one's location and the tax discontinuity, which is more similar to the mechanical tax rate difference using the pre-reform decedents constructed here. In Section 3.5, as a robustness check, we also present the elasticity results using the average tax rate.

3.2 Difference-in-difference Results

2017 Tax Increase. We first examine the event study results for the 2017 estate tax reform, which increased the MTRs for the top 0.5% (see Figure 1). In Panel A of Figure 3, we plot the estimated coefficients and their 95% confidence intervals from the specification (1). The vertical dashed line marks the timing of the reform, which occurred in May 2017. The event study reveals no statistically significant differences in the pre-trends of the treated and control groups, lending support to the parallel assumption. After the tax increase, the reported estates started to decline quickly. They dropped by 18% by the end of 2017 and oscillated around 20% until the end of 2019.

[Figure 3 here]

Table 1 summarizes the diff-in-diff and elasticity estimates. Column (1) presents the results of the first-stage, reduced-form, and implied elasticities for the 2017 tax increase.

The first row of the first stage shows that the treated group experienced a 7.4% decrease in their tax rates compared to the control. The reduced form estimates indicate that they decreased their reported estates by 20.4% after the reform. The implied elasticity is 2.76 (s.e. 0.39), which means that with a 1% decrease in the net-of-tax rate, reported estates decreased by 2.76%.

[Table 1 here]

2009 Tax Cut. We apply the same empirical strategy to the 2009 tax cut, which impacted the top 4%. We define our treated group as decedents with estates above this threshold and use the same control group definition (90th-96th percentiles) as used in the 2017 reform so that we can consistently compare the two reforms.

Panel B in Figure 3 presents the event study of the 2009 tax cut, estimated using the same Specification (1). The vertical dashed line represents the timing of the reform in January 2009. Consistent with the 2017 reform, there are no statistically significant differences in the pretrend of the treated and control groups, supporting the parallel trend assumption. Following the reform, the treated group (top 4%) had a quick increase in their estates, rising by around 8% by the end of 2009. The response continued to grow to nearly 20% by the end of 2011.

Column (2) of Table 1 presents the elasticity estimates for the 2009 reform where the treated group is the top 4%. On average, the treated faced a 10% increase in their net-of-tax rates, and they increased their reported estates by 13.1%. This yields an elasticity of 1.31 (s.e. 0.16). In other words, when the net-of-tax rate increases by 1%, there is a 1.31% increase in the reported estates. Comparing the trends between the 2017 tax increase and the 2009 tax cut, we observe that while responses were immediate in both cases, the response to the 2017 tax increase was stable over time, whereas the response to the 2009 tax cut was more gradual. We will return to this point later when we explore the underlying mechanism behind the asymmetry between the two reforms

Next, we investigate the heterogeneity in the responses across different types and levels of the tax changes induced by the 2009 reform, which raised the exemption threshold from 7.79 to 12 million TWD and replaced the progressive MTRs that previously ranged between 11% and 50% with a flat rate of 10% (s.e.e Figure 1). Due to the increase in the exemption threshold, some individuals have their tax liabilities entirely removed (a repeal), while others have their tax rates lowered to a non-zero rate (a within-positive rate change).

To examine differential responses to these varying types of tax changes, repeal v.s. within-positive rate change, we further split the top 4% treated group into three subgroups: top 4%-2% (repeal), top 2%-0.5%, and top 0.5%. The first group is the group that experienced the repeal, whereas the latter two faced a within-positive rate change. The top 0.5% also serves as a reference group for consistent comparisons with the treated group of the 2017 tax increase. We estimate the same Specification (1) separately for these subgroups where the control group is always those between the 90th-96th percentiles.

Panel C illustrates the evolution of the reported estates of these three subgroups. The estimates of the repeal group are in grey, those of the top 2%-0.5% are in blue, and those of the top 0.5% are in orange. Again, we find no statistical difference in the pre-trends of these groups and the control. In terms of responsiveness, both the repeal and the top 2%-0.5% show rapid increases in their reported estates, with approximately a 10% increase within the first period after the reform. The responses persisted and grew over the years. As for the top 0.5%, the estimates are noisier due to fewer observations. While there is an upward trend in their reported estates in the first two years after the reform, the estimates are not statistically significant. However, starting from 2011, their responses show a salient increase and reach around 30% increase by the end of 2011.

When we consider the magnitude of the tax rate changes they faced (the first stage), the discrepancy across them becomes more salient. Column (3) through Column (5) of Table 1 present the results of the repeal (top 4%-2%) and the within-positive decrease (top 2%-0.5% and top 0.5%), respectively. The three groups experienced very different net-of-tax rate changes, with the repeal group facing the smallest change (4.5%) and the top 0.5% experiencing the largest change (33.1%). In terms of their reduced forms, the 95% confidence intervals for these groups come close. On average, the repeal group increased their reported estates by 13%, the top 2%-0.5% group by 20.4%, and the top 0.5% group by 15.4%. These differences yield different levels of elasticity: the repeal group has an elasticity of 2.89 (s.e. 0.27), while the within-positive rate change groups have elasticities of 1.59 (s.e. 0.17) for the top 2%-0.5% and 0.47 (s.e. 0.13) for the top 0.5%, respectively.

Summary. There are two key observations from Table 1. First, within the 2009 tax cut, there is a heterogeneous response between those who faced a repeal versus those who faced a within-positive rate change, with the repeal having a much larger elasticity. However, a limitation of this comparison is that these groups differ not only in the type of tax change but also in their estate levels. To address this concern and reinforce our argument that the type of tax change drives the differential responses, we turn to an alternative bunching approach in Subsection 3.3. This method allows us to compare kinks of different types that are close to each other in the estate distribution, alleviating concerns about differences in estate levels.

Second, across reforms, there is an asymmetric elasticity for the top 0.5%. The elasticity resulting from the tax increase is almost six times greater than that of the tax cut. Although asymmetric tax incidence has been documented in other contexts (Benzarti et al., 2020, 2024), to our knowledge, it has never been shown in the estate tax literature. In Section 3.4, we compare our elasticity estimates to those in the existing literature and delve deeper into the mechanism of this asymmetry in Section 4.

3.3 Alternative Bunching Approach

We augment our analysis using the bunching method following Saez (2010). This approach allows us to focus on taxpayers near specific kinks in the tax schedule who have similar levels of estates. By comparing behaviors around kinks that are similar in estates but differ

in the type of tax change (exemption kinks versus within-positive kinks), we can better isolate the impact of tax change type on taxpayer behavior.

To make our estimation comparable with the existing studies, we recover the counterfactual distribution near the kink points by fitting a seven-order polynomial and bootstrap the standard errors à la Chetty et al. (2011).

3.3.1 Results

Exemption kinks. First, we examine the exemption kinks and present the results in Figure 4. Panel A illustrates the exemption kink at 12 million TWD in the flat-rate schedule during 2009–2019, associated with a tax rate change from 0% to 10%. Following the existing literature, we construct counterfactual distributions using a seventh-order polynomial, represented by the orange line. We report the excess mass estimate as b=2.68 (s.e. 0.74) and the elasticity estimate as e=0.45 (s.e. 0.12).

In Panel B, we analyze the exemption kink at 7.79 million TWD during the progressive schedule in the 2006–2008 period, associated with a smaller tax rate change from 0% to 2%. The excess mass and elasticity estimates are b=1.99 (s.e. 0.52) and e=2.56 (s.e. 0.66), respectively.

These findings suggest that individuals have a strong incentive to bunch at exemption thresholds. However, the different elasticity estimates imply that individuals do not respond proportionally more with the magnitude of the tax rate change at exemption thresholds, suggesting that there are factors beyond the magnitude of tax rate influencing taxpayer behavior. We discuss this further in Section 3.4.

[Figure 4 here]

Within-positive kinks. Next, we examine the within-positive-kinks in the progressive schedule during 2006–2008. Panels C to F present the density distributions around the fourth through seventh kinks at 12.80, 14.47, 18.92, and 24.49 million TWD, respectively.[5] In all cases, we cannot reject the null hypothesis of no bunching. In other words, the estimated elasticities at these within-positive-kinks are statistically insignificant.

3.4 Discussion: Elasticities

Comparison Diff-in-diff v.s. Bunching. Comparing the estimates from the two approaches, we observe two similarities. First, both methods find quick and persistent responses. In the diff-in-diff results, the event study graphs show the responses began within the first year after the reform and persisted through the last year of the studied period following both reforms. Consistently, as shown earlier in Section 2.3, the bunching results imply that when the exemption threshold shifted, the excess mass also followed quickly and became more salient over time.

[5] We exclude the within-positive-kinks at 8.46 million and 9.46 million TWD because they are too close to the exemption thresholds; as shown in Panel B, there is no bunching around those kinks. For within-positive-kinks above 24.49 million TWD, there are too few observations for credible estimation.

The second similarity is that both approaches indicate that responses to exemption thresholds are bigger than responses to within-positive rate changes. In the diff-in-diff results, the repeal group has an elasticity of 2.89 (s.e. 0.27), which is larger than those of the group who face a decrease to a non-zero rate. In the bunching results, we only detect bunching at the exemption kinks, but not in any other within-positive kinks, suggesting that exemption kinks induce a much stronger response. The elasticity estimates from exemption kinks are sensitive to the magnitude of the tax rate changes used, though. The one estimated from a 0%-10% tax rate change is 0.45 (s.e. 0.12) and the one from a 0%-2% is 2.55 (s.e. 0.66). This suggests that decedents are responding to the exemption kinks not solely because of the tax rate difference, but because there are additional factors beyond the tax rate change. Such a phenomenon aligns with the recent findings in the wealth taxation literature. For example, Garbinti et al. (2023) find that wealth taxpayers respond strongly to kinks associated with information discontinuities (e.g., exemption thresholds or reporting requirement cutoffs) but do not react to within-positive kinks that only affect tax rates. Similarly, Londoño-Vélez and Ávila Mahecha (2024) estimate a larger elasticity using an exemption-notch than a within-positive-notch.

Comparison with Past Literature. Where do our estimates stand in the literature? Table 2 summarizes studies estimating the effects of estate or inheritance tax changes on reported estates or inheritances. We categorize these studies based on the methodologies employed.

Panel A presents estimates using a diff-in-diff method with a repeal variation. Mas-Montserrat (2019) exploits the Catalan inheritance tax repeal on close heirs, comparing estates given to close heirs versus distant heirs, and reports an elasticity of 2.51 (s.e. 0.33), which is close to our repeal estimate of 2.89 (s.e. 0.26).

[Table 2 here]

Panel B reports our estimates using a within-positive rate variation in a diff-in-diff setting. To our knowledge, no paper has used a diff-in-diff estimation with a within-positive rate variation to identify the elasticity of estates with respect to the net-of-tax rate. Our contribution to the literature includes not only adding estimates using such an approach but also examining the direction of the rate change by splitting reforms into tax increases and decreases.

Panel C presents the estimates using bunching at the exemption threshold. The only existing study in this area is Escobar et al. (2019), who examines the Swedish inheritance tax repeal and estimates bunching at one exemption kink and two within-positive kinks. They report an elasticity of 1.53 (s.e. 0.10) for the exemption kink. Our bunching estimates from an exemption kink vary between 0.57 (s.e. 0.10) and 3.17 (s.e. 0.51). Although their estimate lies within the range of ours, the mixed results suggest that individuals do not respond proportionally to the magnitude of the tax change at exemption thresholds, as there are factors beyond the rate change itself that influence their behavior.

Panel D displays the estimates from within-positive kinks, the results are also mixed. While Glogowsky (2021) and one of the estimates of Escobar et al. (2019) are statistically

significant, the other estimate of Escobar et al. (2019) and all our estimates are statistically insignificant. Despite these mixed findings, the results are consistent on one point: estimates from within-positive kinks are much smaller than those from exemption kinks and also smaller than those derived from the diff-in-diff estimation.

Panel E includes one estimate using an RDD from Escobar (2017) at 0.88 (s.e. 0.36). Panel F presents the estimates from an older wave of literature using time-series variation and U.S. data. The results are mixed, with Joulfaian (2006) reporting an estimate of 0.14 (s.e. 0.05) that is statistically significant and Kopczuk and Slemrod (2001) having noisy estimates spanning from -0.11 to 0.09 that are statistically insignificant.

To summarize, our estimates contribute to the literature in the following ways. As shown, existing studies report a wide range of elasticity estimates, from statistically insignificant values (Kopczuk and Slemrod, 2001) to as high as 2.51 (Mas-Montserrat, 2019). A possible explanation for these differing findings lies in the types of variation and methods used. We show this is the case by estimating responses using both variations and methods within the same context, helping to reconcile the differences in elasticity estimates in the literature.

In addition, we offer estimates using new variations not previously available in the literature. Specifically, by distinguishing between responses to tax increases and cuts, we show asymmetric responses. We explore this novel aspect and investigate its underlying mechanism in Section 4.

3.5 Robustness Checks

Thus far, we have presented the main results of the behavioral responses in reported estates to estate tax changes. Here we present robustness checks of our main diff-in-diff design.

Stability of Algorithm. One of the identifying assumptions is that the performance of the prediction algorithm is similar before and after the reform. This cannot be tested directly, but we can study pre-reform features of the prediction algorithm in the spirit as we usually test parallel-trend assumptions using pre-reform data.

For the 2017 reform, Appendix Figure B.2 displays the top 25 important predictors used in the algorithm, split by period. In both panels, the same predictors emerge as most influential, with the top five being total wealth values four to six years prior and land values four years before death. Although the order of these predictors varies slightly, they consistently remain the top influential factors. To assess the stability of the algorithm's predictive performance, Appendix Table B.2 presents results by year. Columns (1) and (2) show the overall precision and recall rates, while Columns (3) through (8) provide performance by specific years. Overall, performance across groups shows little variation, supporting the algorithm's stability. Similarly, Appendix Figure B.3 and Table B.3 present the stability of the top predictors and prediction performance for the 2009 reform, respectively.

Placebo Reform Years. To show that the treatment effects we captured in the results are not driven by algorithmic bias, mean reversion, or aspects unrelated to the reforms, we

randomly assigned years as placebo reform years. For a given placebo reform year, we implement the prediction algorithm and diff-in-diff estimation as described in Section 3.1. Appendix B.4 describes the procedures using 2012, 2013, 2014, and 2015 as placebo years. Appendix Table B.5 presents the results. We find no treatment effect in any of the placebo years.

Alternative Control Group. Since the 2009 reform affected the top 4%, in both our baseline estimations of the two reforms, we use those whose predicted estate is between the 90th and 96th percentiles as the control so that we can consistently compare the two reforms. The choice of the control group involves a tradeoff: selecting a control group whose reported estate evolves similarly to the treated group (closer to the top percentiles) while avoiding contamination by the reform (lower down in the percentiles).

In Appendix Table B.4, we show the elasticity estimates when using alternative control groups. For the 2017 reform, the elasticity estimates do not differ much when using P90-P93, P93-P96, and P96-P98. The estimate becomes insignificant when using the immediately below group P98-P99.5 as the control. This is because the group also decreased their reported estates after the tax increase, potentially because they behaved in a forward-looking manner and chose not to cross the threshold, which is consistent with the findings in Garbinti et al. (2023). For the 2009 reform, the estimates are similarly consistent with the P85–P90 control group, except again for the immediately below group.

Anticipation. If individuals expected the government to change the estate tax schedule, they could have adjusted their behavior prior to the policy change. If such anticipation exists, the naive treatment effect would be smaller than the true treatment effect. In Appendix B.5, we assumed the reform took place a year earlier than the true reform year (2016 for the 2017 tax increase and 2008 for the 2009 tax cut). Then, we implement the prediction algorithm and diff-in-diff estimation as described in Section 3.1. Appendix Table B.6 reports the elasticity estimates. In both reforms, the results are similar to the baseline estimates.

Alternative Specifications for Elasticities. In addition to using marginal tax rates to estimate the elasticity of reported estates with respect to the net-of-marginal-tax rate, we conduct a robustness check using average tax rates. This approach addresses the concern that taxpayers may respond to changes in their overall tax burden rather than only to marginal incentives. Significant tax reforms can substantially impact average tax rates and therefore influence estate planning decisions. By estimating elasticities with respect to the net-of-average-tax rate, we assess whether our key findings are robust to the choice of tax rate measure.

As shown in Appendix Table B.7, the estimated elasticities using the average tax rate are greater than those estimated with the marginal tax rate, as the first-stage effects are generally smaller. However, our two main findings (i) heterogeneous elasticities across types (repeal vs. within-positive) and (ii) directions (tax increase vs. cut), remain consistent, which reinforces the validity of our conclusions.

Macro Economic Conditions. One of our main findings is that we observe a stronger response to the tax increase than to the tax cut. A potential explanation could be the macroeconomic context, as the financial crisis coincided with the 2009 tax cut, while the economy was relatively stable during the 2017 tax increase. If this difference matters, one could argue that the smaller response to the 2009 tax cut reflects individuals being constrained in adjusting their assets amid a collapsing financial market. We provide three explanations to show this is not the case.

First, as shown later in Section 4.1, when we break down estate items, deposit savings, a liquid and movable asset that can be adjusted even during a recession, are still valued differently between a tax increase and a cut.

Second, if the concern is the stock market being "illiquid" during the recession, we show that trading in the Taiwanese Stock Exchange rebounded relatively quickly during the financial crisis. As shown in Appendix Figure B.4, trading volume dipped towards the end of 2008 but began to recover in the first quarter of 2009. By April 2009, trading volume had returned to pre-crisis levels, suggesting that the stock market was still very dynamic at the time.

Third, to further support our argument, we use individuals' pre-reform stock holding fraction to proxy their exposure to the financial crisis. We classify the top 0.5% of decedents into two groups: those with a stock holding fraction above the mean (highly exposed) and those below the mean (less exposed), and we estimate the specifications in Equations (2) and (3) separately for each group. Appendix Table B.9 presents the elasticity estimates for each group. The results indicate that the 95% confidence intervals overlap and there is no differential response based on stock holding fraction, lending support to the argument that exposure to the financial crisis does not explain the observed asymmetry in responses.

Gifting. One possible explanation for the observed changes in reported estates following the reforms is a behavioral shift in gifting, as individuals may have increased or decreased their gifts, thus affecting their reported estates at death. As outlined in Section 2.1, the Taiwanese estate and gift tax systems are closely linked, with two key provisions designed to prevent tax avoidance through gifting. First, all gifts made within two years before death are included in the estate tax base. Second, the gift tax schedule moves with the estate tax schedule, i.e., gift and estate taxes are raised or lowered simultaneously. These rules are designed to lower potential estate tax avoidance through gifting near death.

The goals of this robustness check are twofold. First, we show that changes in reported estates after both reforms are not due to changes in gifting. Second, we analyze the incentives involved by considering the costs of estate, gifting, and other avoidance strategies, as well as an individual's expected survival within two years (as this affects whether gifts count toward the estate tax). This allows us to explore the tradeoffs between these factors and test if these mechanisms hold empirically.

Changes in gifting do not attribute to changes in estates. For the first goal, we estimate Equation 3 with gifts made within two years before death, scaled by pre-reform

average wealth. Appendix Table B.10 presents the results: Column (1) shows that, after the 2017 tax increase, decedents reduced gifts by about 2.7%, suggesting that decreased estates are not due to increased gifting, as we would expect an increase in gifting if it were driving the decrease in reported estates. Column (2) indicates no significant change in gifting after the 2009 tax cut, supporting that gifting does not explain the increased estates.

Mechanism and predictions. Why do we see different gifting responses between the two reforms? We analyze the incentives in each reform, as shown in Appendix Table B.11. Gifting responses depend on: (i) the relative cost of leaving an estate versus gifting, (ii) the relative cost of gifting versus other tax avoidance strategies. In addition, these channels differ in whether individuals expect to live or die within the two-year window.

Panel A in Appendix Table B.11 outlines the mechanisms underlying the 2017 reform, where both estate and gift taxes increased. For individuals expecting to die within two years, gifting provides no tax advantage, as any gifts made within this timeframe are included in the estate tax base. As a result, the relative cost between estate and gifting does not matter. However, the increased cost of gifting relative to other avoidance strategies matters, leading individuals to reduce gifting in favor of other strategies. Overall, we predict that these individuals will gift less.

For those expecting to live longer than two years, the effect is less clear. The increased relative cost of gifting versus leaving an estate creates a mixed incentive: they may choose to gift more to reduce a now higher future estate tax, but this incentive is reduced by the higher cost of gifting. In addition, the increased cost of gifting relative to other avoidance strategies may discourage gifting. The combined effects of these two channels result in an ambiguous impact on gifting behavior.

Panel B discusses the predictions of the 2009 reform when both estate and gift taxes decreased. For individuals expecting to die within two years, the relative cost between estate and gifting remains irrelevant. Although the cost of gifting relative to other avoidance strategies is now lower, it is unlikely that individuals will switch from other avoidance strategies to gifting, as gifts will be counted toward the tax base. Therefore, we expect no change in gifting behavior.

For those expecting to live longer than two years, the relative cost between estate and gifting yields an ambiguous effect: they may choose to gift more to reduce future estate tax liability, but the lower cost of leaving an estate could reduce the incentive to gift now. Regarding the relative cost between gifting and other avoidance strategies, the lower gift tax encourages a shift from other strategies to gifting, which could lead to increased gifting. The combined effect of these channels is thus ambiguous.

We test these predictions in Appendix Table B.12, using sudden death as a proxy for individuals' expectations of survival[6]. The results of the 2017 tax increase are in Columns (1) and (2). Nonsudden deaths (those likely expecting to die within two years) reduced

[6]We define a death being sudden if the cause of death is one of the following: traffic accidents, natural disasters, lightning strikes, workplace accidents, acute asthma, strokes, choking, drowning, fire.

gifting by about 2.6%, while sudden deaths showed no change. For the 2009 tax cut, shown in Columns (3) and (4), nonsudden deaths showed no change in gifting, while sudden deaths showed a 4% increase. This increase suggests that the combined effect of gifting to reduce estate and shifting from other avoidance strategies to gifting due to the lowered gift tax dominates the reduced cost of leaving an estate. These findings are consistent with our predictions.

4 Mechanism

We have shown in the previous section that reported estates respond quickly and persistently following both reforms and that there is an asymmetrically stronger response to a tax increase than a tax cut. In this section, we aim to understand the sources of the responses and the driver of the asymmetry.

In Section 4.1, we break down estates by items to examine which components contribute to the responses. We calculate the elasticities of each estate item with respect to net-of-tax rates and quantify each item's contribution to the total change in reported estates.

Then, in Section 4.2, we examine whether the responses are tax avoidance or real behaviors such as adjusting labor supply or consumption by investigating decedents' pre-death outcomes and their heir children's labor supply after the reforms.

4.1 The Anatomy of Estate Items

What are the sources of the responses, and what drives the asymmetrical responses between a tax increase and a cut? To consistently compare the two reforms, we focus on the top 0.5% decedents and break estates into:

$$\begin{aligned}\text{Estates} = &\underbrace{(\text{Housing} + \text{Financial Asset} + \text{Deposit Savings})}_{\text{Asset}} \\ &- \underbrace{(\text{Other Deductible} + \text{Charitable \& Exemptions})}_{\text{Deduction}}\end{aligned}$$

Elasticities of Each Item. We estimate the elasticity of each item following the same procedure described in Section 3.1, replacing the dependent variable in Equation (3) with the value of each item. We then scale the coefficient by the mean of the item for the treated group in the pre-reform period. Figure 5 illustrates the elasticity estimates by item by reform. Appendix Table C.1 and C.2 present the detailed first-stage, reduced-form estimates used to calculate them. In Appendix Figure C.1 and C.2, we also show the event study graphs of the dynamic evolution of these items.

[Figure 5 here]

The blue estimates in Figure 5 refer to the results of the 2017 tax increase. We observe no response in housing and other deductibles, while financial asserts, deposit savings, and charity and exemptions shifted. The estimated elasticities of these items with respect to net-of-tax rate are 2.79 (se 0.35) for financial assets, 1.87 (se 0.43) for deposit savings, and 2.39 (se 0.67) for charity and exemption.

The orange estimates refer to the results of the 2009 tax cut. We find small responses in deposit savings and other deductibles and a greater response in charity and exemptions. The estimated elasticities are 0.38 (se 0.17) for deposit savings, 0.18 (se 0.09) for other deductibles, and 0.80 (se 0.15) for charity and exemption.

Comparing the sources of responses between the two reforms, we find that the 95% confidence intervals of the estimated elasticities differ significantly in financial assets, deposit savings, and charity and exemptions, suggesting they are the drivers of the asymmetrical responses.

Decomposition of Contribution to Total Elasticity. How does each elasticity of the item contribute to the elasticity of the total reported estate? We decompose the elasticity into a weighted sum of the elasticities of items.

$$\varepsilon^{Estate} = \sum_{Item} \varepsilon^{Item} \times \omega_{\frac{Item}{Estate}}$$

The contribution of each item to the total elasticity of the reported estate depends on: (i) the magnitude of its own elasticity and (ii) the weight of the item in total estate. Table 3 presents the decomposition where we report the elasticity, weight, and the product of each item.

Panel A shows the numbers for asset items. Although liquid assets are more elastic, their smaller weight results in a lower contribution to the overall change in estate values. A similar pattern is observed for charitable deductions and exemptions in Panel B: while these items exhibit the highest elasticity, their small weight means they contribute relatively little to the total change.

For the 2017 tax increase, 66% of the change in reported estates is driven by assets, with the remaining 34% coming from deductions. In contrast, during the 2009 tax cut, only 4% of the change is driven by assets, while 96% comes from deductions. This suggests that most of the estate response during the tax increase is attributable to changes in assets, whereas the response during the tax cut is largely driven by changes in deductions.

[Table 3 here]

Summary. To summarize, first, we find that housing, an illiquid and third-party-reported item, is unresponsive in both reforms. This is consistent with the findings in the tax literature that link tax avoidance to whether items are third-party-repoted (Kleven et al., 2011; Waseem, 2020; Londoño-Vélez and Ávila Mahecha, 2024).

Second, the asymmetry between the two reforms is driven by more liquid and adjustable

items: financial assets, deposit savings, and charity and exemptions. From a liquidity perspective, it is expected that these movable items would show greater responses in a shorter period. However, it remains unclear whether these shifts are avoidance behaviors, such as setting up trust funds or moving assets offshore, or real behaviors such as changes in consumption and labor supply. Given that the responses occurred quickly– within a year, we already see changes in reported estates, we argue these responses are more likely driven by avoidance. To explore this possibility further, the next section explores decedents' asset evolution before death, as well as labor supply behaviors by both decedents and their heirs.

4.2 Are the Responses "Real"?

Building on the findings above, this section aims to investigate whether the responses reflect tax avoidance or real behavioral changes. The changes in reported estates of decedents could be adjusted through several channels. Taking the 2017 tax increase as an example, the decrease in reported estates could be coming from decedents (i) consuming more; (ii) changing labor supply; (iii) using tax avoidance instruments such as setting up trust funds or exploiting offshore accounts to shift assets to get away from taxes etc.

Depending on what channels decedents adjust to, it will have different impacts on the heirs. If decedents change their consumption or labor supply, which in turn affects the actual estates left to their heirs, heirs might show corresponding behavioral responses. On the contrary, if decedents primarily engage in tax avoidance strategies that change reported but not true estate values, heirs' behavior may remain unaffected.

To examine this, Section 4.2.1 analyzes the pre-death wealth outcomes of decedents, focusing on variables such as housing, stock holdings, and labor income. Changes in the former two can indirectly signal changes in consumption or saving behavior, while the latter shows changes in labor supply.

In Section 4.2.2, we turn to heirs and examine the effect of the tax changes on their inheritance and labor supply behavior. *A priori*, if the changes in reported estates among decedents are primarily driven by tax avoidance, we would not expect to observe significant effects on the labor supply of their children.

4.2.1 The Effect of Reforms on Decedents' Outcomes Before Death

Housing and Stock holding. We use the overlapping items, housing and stock holding, which we observe both in the estate (at death) and wealth (pre-death) data to benchmark how much of the change in reported values at death can be attributed to changes in lifetime behavior after the reform. We focus on the outcomes one year and two years before death and estimate the same Equation 3 where we scale the outcome variables with the average pre-reform wealth. The results are shown in Table 4.

We first focus on the results of the 2017 tax increase, which are presented in Columns (1) to (3) in the table. Panel A Columns (1) to (3) are the housing values reported at death, one year before death, and two years before death, respectively for the 2017 tax increase. It shows that pre-death housing values did not change after the reform, which is consistent with the null response in reported housing at death.

[Table 4 here]

The relationship between financial assets reported at death and stock holding is trickier because financial assets include stock holding plus other self-reported items, and in the estate data, we only see its aggregate level. In Panel B, we report the effect of the reforms on financial assets reported at death. Column (1) shows that the 20.7% drop in financial assets (Column (2) in Panel B in Appendix Table C.1) translates into a 15.4% decrease when we measure it relative to the average pre-reform wealth and not to the average of its own pre-reform value.

In Panel C, Columns (2) and (3) report the stock values one year and two years before death, respectively. We find that the decline in reported financial assets at death during the 2017 tax increase is reflected in pre-death stock values. Stock values decreased by around 4% one and two years before death after the 2017 tax increase.

To investigate further, we split stock holding into privately- and publicly-listed stock, presented in Panel D and E, respectively. It shows that privately-listed stock decreased by around 2.5% before death. Publicly-listed stock decreased by around 1%. However, its decrease is only marginally statistically significant one year before death and statistically insignificant two years before death. In summary, the decline in stock values before death is mostly driven by privately-listed stock. This type of stock holding is assessed based on the net value of the firm, and these firms are often closely held by estate taxpayers who have significant control over firm valuations.

Benchmarking the extent to which the decrease in reported financial assets at death can be explained by the drop in pre-death stock requires additional assumptions, as the estate data only provides aggregate financial asset values without specifying the stock component. If we assume the extreme case that all financial assets are stock, this means $0.041/0.154 = 26.7\%$ of the decline in reported estate values can be attributed to changes in stock holdings one year before death. The rest would be a mixture of consumption/saving adjustments and avoidance manipulation made in the final year of life.

Columns (4) to (6) in Table 4 present the results of the 2009 reform. We do not find a change in one's lifetime housing and stock values, which is consistent with results in Section 4.1 where most of the increase in reported estates arises from deductions rather than assets.

Labor Income. Panel F in Table 4 displays the change in labor income. We find no effect on decedents' labor income one and two years before death following both reforms. This implies that labor supply was not a channel through which decedents adjusted their estates reported at death.

Summary. In both reforms, we can rule out the labor supply channel as a driver of changes in reported estates, as we find no effect on labor income. The remaining channels affecting reported estates are consumption/saving adjustments and tax avoidance.

During the 2017 tax increase, it is shown that individuals decreased their privately-listed stock holding, which resulted in a drop in the financial assets reported at death.

Disentangling whether the drop is a "real" change or tax avoidance is challenging and requires a more detailed analysis of firms' dividend payout behaviors; however, considering that many of these estate taxpayers closely hold these firms with substantial control over firm valuations, as well as the quickness of the response, we interpret this as more likely to reflect tax avoidance. In the following Section 4.2.2, we examine the heirs' outcomes to further investigate this hypothesis.

4.2.2 Heirs' Outcomes

We use decedents' heir children to examine the effect of the tax changes on their inheritance and their subsequent labor supply behavior. First, we establish a strong first stage by showing that the tax changes led to significant differences in inheritance levels. We then examine how these changes affect heirs' labor supply before and after reforms.

To construct the heir children samples, we start with the decedent samples and restrict them to the decedents who (i) did not have a living spouse at death and (ii) whose spouses did not die within three years before them.[7]

First stage: Inheritance. To examine how much their inheritances changed because of the tax changes, we estimate the following cross-sectional regression:

$$\begin{aligned}\text{Inheritance}_{j(i)} = \alpha_0 + \alpha_1 \text{Treated}_{j(i)} \times \text{PostReform}_i + \alpha_2 \text{Treated}_{j(i)} \\ + \alpha_3 \text{PostReform}_{j(i)} + \epsilon_{j(i)}\end{aligned} \tag{4}$$

where j is heir (child) and i is decedent (parent). $Inheritance_{j(i)} = Asset_i / NumHeir_i$[8], which is the average asset values of decedent i per heir. $Treated_{j(i)}$ is 1 if heir j's parent i is defined as treated in the decedent specification, which is when decedent i is predicted to be above the top 0.5% and 0 if between the 90th and 96th percentiles; $PostReform_{j(i)}$ is 1 if heir j's parent i died after the reform. The coefficient of interest is α_1, which is the treatment effect of the reforms on inheritance.

Table 5 presents the first-stage result. On average, the treated heir received 15 million TWD (about 480K USD) less after the 2017 tax increase and received 9 million TWD (about 340K USD) more after the 2009 tax cut. The difference between the 2017 and 2009 reforms reflects our earlier findings, where the 2017 tax increase induced a larger change in asset values compared to the 2009 tax cut.

[Table 5 here]

Labor Supply. The substantial first stage suggests that the reforms have a non-trivial impact on heirs. We now ask what the effect of a change in inheritances on their labor supply behavior is. To examine this, we track them three years before and four years

[7] The first restriction ensures that the inheritance does not go solely to the spouse, as we do not see how the estate is split among the heirs. The second restriction ensures that the heir children are not doubly shocked in a short period of time.

[8] $Asset_i$ is the reported assets of decedent i from the estate data. As mentioned earlier, gifts made within two years before death are also included, when constructing this number, we net out the gifts made within two years. $NumHeir_i$ is the number of children heirs of decedent i.

after their parent's death. We estimate the following individual fixed-effects regression separately for heirs whose parents died before and after the reform:

$$\begin{aligned} y_{j(i),t} = &\sum_{k\neq -1} \beta_k \cdot \text{Treated}_{j(i)} \times \mathbb{1}[t - \text{DeathYear}_i = k] \\ &+ \sum_{k\neq -1} \mathbb{1}[t - \text{DeathYear}_i = k] + \gamma' X_{j(i),t} + \alpha_{j(i)} + u_{j(i),t} \end{aligned} \tag{5}$$

where $y_{j(i),t}$ is the outcome of heir j in time t. $Treated_{j(i)}$ is defined as before, which is 1 if heir j's parent is predicted to be the top 0.5% and 0 if between the 90th and 96th percentiles. $\mathbb{1}[t - ParentDeathYear_i = k]$ are year-to-parental-death time dummies. $X_{j(i),t}$ contains age-specific fixed effects and marriage status that vary within an heir across years. $\alpha_{j(i)}$ is heir fixed effect. We omit event time $k = -1$ and cluster the standard error at the heir level.

Panel I of Figure 6 presents the results of the 2017 tax increase. Panel I(A) plots the evolution of the probability of being employed after the parent's death. The estimates for those whose parents died before the 2017 tax increase are in grey and those after are in blue. For those whose parents died before the tax increase, the probability of being employed dropped by around 2.5% one year after the parent's death and decreased to around 2% after four years. In other words, when exploiting the variation in the amount of inheritance between the treated (children of the top 0.5%) and control (children of those between P90-P96th percentiles), we find that the treated heirs reduced their labor supply more on the extensive margin. This also applies to those whose parents died after the 2017 tax increase.

When comparing these two samples, which is to exploit the tax variation where those who died after the tax increase now faced a higher tax liability, we show that the 95% confidence intervals of the two samples overlap and exhibit no statistically significant difference. This indicates that there is no difference in the extensive margin before and after the tax increase.

[Figure 6 here]

Panel I(B) plots the results of log labor income, which is the intensive margin. For both samples, treated heirs do not exhibit a different trend on their intensive margin compared to the control group. When comparing the two samples before and after the reform, we again find no difference between those whose parents died before versus after the reform.

For the 2009 tax cut, Panel II(C) demonstrates the results for the extensive margin. The probability of being employed of those whose parents died before the 2009 tax cut was reduced by around 0.5% within the first year after parent's death and further decreased to around 2% after four years. A similar pattern is observed for heirs whose parents died after the tax cut, suggesting that treated heirs respond more on the extensive margin than the control because of the levels of inheritances they receive. However, comparing the pre- and post-reform samples, we again do not see a statistically significant difference in whether

the parents died before or after the reform. Similarly, Panel II(D) aligns with the findings of the 2017 tax increase, showing no differential behavior on the intensive margin between treated and control groups, nor between heirs whose parents died before and after the reform.

Summary and Comparison to Past Literature. Our specification Equation (5) exploits two variations: (i) the level of inheritance between the treated (children of the top 0.5%) and control (children of the P90-P96th), and (ii) the level of inheritance induced by the estate tax reforms.

For the first variation, we find that treated heirs reduced their labor supply on the extensive margin but not on the intensive margin. This result aligns with the existing literature, where most studies report a negative effect on the extensive margin following a parent's death (Holtz-Eakin et al., 1993; Nekoei and Seim, 2023). However, results on the intensive margin are mixed. Nekoei and Seim (2023) report around a 1% drop in earnings, while Holtz-Eakin et al. (1993) find no effect for single filers and only a small negative effect for joint filers,[9] with earnings decreasing by less than $1,000 for a $1 million USD inheritance.

The second source of variation, derived from estate tax changes, is particularly relevant because it is likely more exogenous than inheritance received following a parent's death and has not been extensively explored in previous literature. Using this exogenous variation, we find that no effect on heirs' labor supply before and after the estate tax changes. This null effect is striking given the substantial first-stage response observed in the change in the level of the inheritance receipt. The lack of an effect on heirs' labor supply, despite a significant first stage, combined with the rapid changes in decedents' reported estates and evidence from Section 4.2.1 supports a tax avoidance narrative. If changes in decedents' estates mainly reflect tax avoidance rather than true adjustments in estate values, heirs would have little reason to alter their behavior before and after the reforms.

4.3 Discussion: Asymmetrical Tax Avoidance Responses in the Presence of Sunk Costs

These findings, along with the quick changes in the reported estate that responded within a year after the reforms, suggest that the decedents' responses we capture are likely to be driven by tax avoidance rather than real changes. The asymmetrical responses between the tax increase and the tax cut also fit this interpretation. As individuals engage in tax planning during their lifetime, they incur avoidance costs such as setting up offshore accounts, establishing trust funds, or paying professional tax planners. When taxes increase, they may increase their avoidance even more. However, when taxes decrease, they may not reduce their avoidance by as much since they have already invested in these avoidance instruments. This also explains the faster response observed during the 2017 tax increase compared to the gradual adjustment seen in the 2009 tax cut.

[9] Holtz-Eakin et al. (1993) report that a $1 million USD increase in inheritance reduces joint filers' earnings by less than $1 thousand USD.

Although we do not have data on individuals' behavior about setting up trust funds or transferring wealth offshore, we show aggregate-level evidence that highlights the prevalence of these avoidance strategies among wealthy Taiwanese individuals. Appendix Figure C.4 shows the growth of aggregate trust fund sizes in Taiwan over time, as reported by the Taiwanese Trust Association. Interestingly, there was a sharp increase after the 2017 estate tax increase, with trust fund sizes growing by 43% between 2017 and 2020.

Offshore wealth plays a particularly significant role in Taiwan. According to Alstadsæter et al. (2018), the fraction of offshore wealth relative to GDP in Taiwan ranked 7th globally. Appendix Figure C.5 shows that offshore wealth in Taiwan accounted for 22% of GDP in 2007, substantially higher than the global average of 9.8%. Even more striking, Appendix Figure C.6 shows Taiwan topping the list for the number of unique shell company owners identified in the Panama Papers Leak.

These pieces of evidence suggest that wealthy individuals in Taiwan have access to means for avoiding/evading estate taxes. These avoidance measures often come with setup costs such as legal fees or administrative expenses that become "sunk" once investments are made. Reversing these arrangements by bringing money back onshore or dissolving trusts can also be costly or carry penalty fees, making individuals less inclined to adjust avoidance efforts when tax rates decrease, which is why there is an asymmetrical response to tax increases versus decreases.

5 Welfare Analysis

In this section, we propose an estate tax planning framework built on Chetty (2009). We first start with a static framework, then explain the observed asymmetry in the empirical section with a two-period scenario, and analyze the welfare effect when the tax rate changes.

5.1 Model Setup

Consider an individual i who has an initial wealth w. She chooses to consume c, shelter s units of wealth by paying a resource cost $\Gamma(s)$, and leave e as estates to her heirs, which are the after-tax reported estates plus the amount she sheltered. Her reported estates are $ReportedEstate \equiv w - c - s$. Her true estates left are the after-tax reported estates plus the sheltered amount, $e \equiv (1-\tau) \cdot ReportedEstate + s$ where $\tau \in [0, 1]$. $\Phi(e)$ is the utility of bequeathing estates to her heirs. We assume agents have quasi-linear utility.

She solves the following maximization problem

$$\max_{c,s} \quad c + \Phi(e) - \Gamma(s)$$

$$\text{s.t.} \quad \underbrace{e}_{\text{True Estates}} = \underbrace{(1-\tau) \cdot (w-c-s)}_{\text{After-tax Reported Estates}} + \underbrace{s}_{\text{Sheltered amount}}$$

The F.O.C. are

$$\partial/\partial c : (1-\tau)\cdot\Phi'(e) = 1$$
$$\partial/\partial s : \tau\cdot\Phi'(e) = \Gamma'(s)$$

The first F.O.C. implies that the individual will choose consumption such that the after-tax marginal utility of leaving estates is equal to the marginal utility of consumption. The second F.O.C. indicates that the individual will shelter up to the point where the marginal benefit of sheltering is equal to the marginal cost of sheltering.

Explanation for the Asymmetry. In our empirical results, we find that individuals respond more strongly to tax increases than to tax cuts. This phenomenon can be explained by a two-period model in which the government unexpectedly changes the tax rate in the second period.

In the second period, individuals can adjust their sheltering decisions by either increasing or decreasing the amount of sheltered wealth. However, the marginal cost of decreasing what was already sheltered would be higher than increasing it. For example, reducing sheltered assets may involve paying penalty fees for dissolving trust funds or shifting money back onshore.

As a result, the model would predict that when there is a tax increase, individuals will choose to shelter more wealth in the second period. In contrast, when there is a tax cut, individuals are less likely to reverse their initial sheltering decisions, leading to a smaller response. This asymmetry explains why we observe a larger behavioral response to tax increases compared to tax cuts.

Welfare. How does welfare change in response to tax rate changes? Suppose welfare is the sum of agents' utilities and tax revenues:

$$W(\tau) = \underbrace{c + \Phi(e) - \Gamma(s)}_{\text{Utility}} + \underbrace{\tau\cdot(w-c-s)}_{\text{Tax revenues}}$$

$$dW(\tau) = \underbrace{\frac{-RE\cdot d\tau}{1-\tau}}_{dU} + \Big[\underbrace{RE\cdot d\tau}_{dM} - \underbrace{\frac{\tau\cdot\varepsilon\cdot RE\cdot d\tau}{1-\tau}}_{dB}\Big]$$

where the first term dU is the change in utilities, which is the direct effect of leaving less estate to heirs. The second term is the change in tax revenues, which could be further split into a mechanical change (dM) and behavioral change (dB).

5.2 Policy Analysis

Using our empirical estimates, we explore the policy implications of estate tax reforms by focusing on the top 0.5%. This allows us to compare outcomes under different scenarios by applying the estimated elasticities from the 2017 tax increase and the 2009 tax cut and

compare the scenarios under whether accounting for the presence of sunk costs.

In our empirical findings, we find that responses to tax increases and cuts differ in both magnitude and adjustment speed: the response to the 2017 tax increase is immediate and stable, while the response to the 2009 tax cut evolves gradually, beginning smaller and increasing as individuals recover from sunk costs. This implies that during a tax increase, the elasticity without sunk costs will accurately inform the welfare effect. In contrast, during a tax cut, both elasticities, the short-run and the long-run, will inform welfare, with the rate at which individuals recover from sunk costs impacting the cumulative welfare effect.

2017 Tax Increase. Columns (1) and (2) of Table 6 present the parameters and results for the 2017 reform, utilizing the estimated elasticities from the 2009 tax cut and the 2017 tax increase, respectively. Panel A includes the average marginal tax rate, the average tax rate change, and the total sum of reported estates of the top 0.5% in 2016 observed in our data.

Panel B presents the results of tax revenue changes, splitting by the mechanical and behavioral effects. dB/dM could be interpreted as, with \$1 TWD mechanically raised from the tax increase, how much would be lost due to behavioral effect. Panel C illustrates the welfare changes.

In Column (1), using the 2009 tax cut elasticity suggests that for each 1 TWD mechanically raised, there is only a 0.05 TWD loss from behavioral effects, resulting in an increase in tax revenues by 10.6 billion TWD and a welfare decrease 1.8 billion TWD. In contrast, Column (2), which uses the 2017 tax increase elasticity, indicates a welfare loss of 4.6 billion TWD due to a more substantial behavioral response.

The discrepancy between the two scenarios highlights that the welfare change would be overestimated by 61% if the government naively implements the underestimated elasticity where sunk costs are present.

2009 Tax Cut. Columns (3) and (4) of Table 6 summarize the parameters and results of the 2009 tax cut. Panel A shows the data moments of the top 0.5% in 2008 observed in our data.

In Column (3), applying the estimated elasticity of the tax cut (0.465) represents the short-term welfare impact. This results in a decrease in tax revenues by 12.53 billion TWD and an increase in welfare by 12 billion TWD. In contrast, Column (4) uses the higher elasticity estimate from the 2017 tax increase (2.757), which represents the long-term effect. This leads to a much larger behavioral effect that offsets the mechanical decrease in tax revenues, resulting in a welfare increase of 31 billion TWD.

This suggests that during a tax cut, in the short run, as individuals face sunk costs, tax revenues will decrease; however, as they recover from the sunk costs, tax revenues will ultimately flip sign and become positive. The cumulative tax revenue change will depend on the pace at which individuals recover from their sunk costs.

6 Conclusion

This study leverages two large estate tax reforms in Taiwan to analyze behavioral responses to estate taxation. We find that responses are both quick and persistent, and differ substantially based on the nature of the tax change: full repeals elicit larger elasticities than within-positive tax variation, and responses to a tax increase are six times stronger than those to a tax cut.

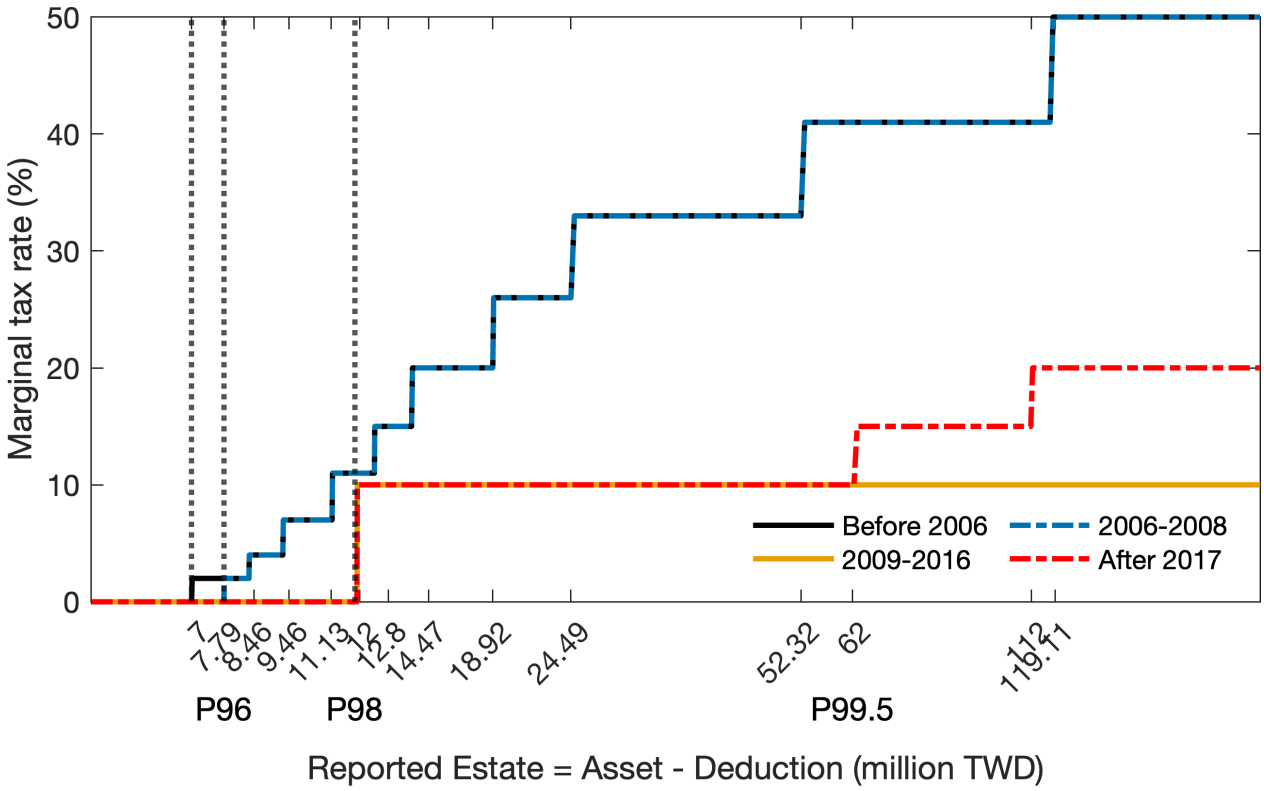
Our analysis suggests that these adjustments are mainly driven by tax avoidance, particularly through liquid assets like financial holdings and charitable exemptions, which respond more strongly to tax increases. The lack of labor supply changes among decedents and heirs supports the interpretation that these responses reflect avoidance strategies rather than real behavioral adjustments.

The observed asymmetry in responses aligns with the tax avoidance framework: initial investments in avoidance strategies create sunk costs, leading individuals to adjust more to tax increases than to tax cuts. We model these dynamics and show that using elasticities from scenarios with sunk costs could misestimate welfare impacts. If the government applies the tax cut elasticity, which is dampened by sunk costs, it would underestimate the welfare cost and overestimate the net welfare effect of a tax increase by 61%.

Our findings have important policy implications. First, the substantial tax avoidance in response to estate tax changes underscores the need for stronger enforcement measures, such as expanding third-party reporting and enhancing international cooperation to detect offshore assets. Second, the observed asymmetry in taxpayer responses to estate tax increases and cuts has significant welfare implications and should be carefully considered by policymakers when evaluating estate tax reforms. Moreover, this asymmetry due to sunk costs may extend beyond estate taxes to other areas where tax avoidance involves similar fixed costs, such as corporate income and wealth taxes.

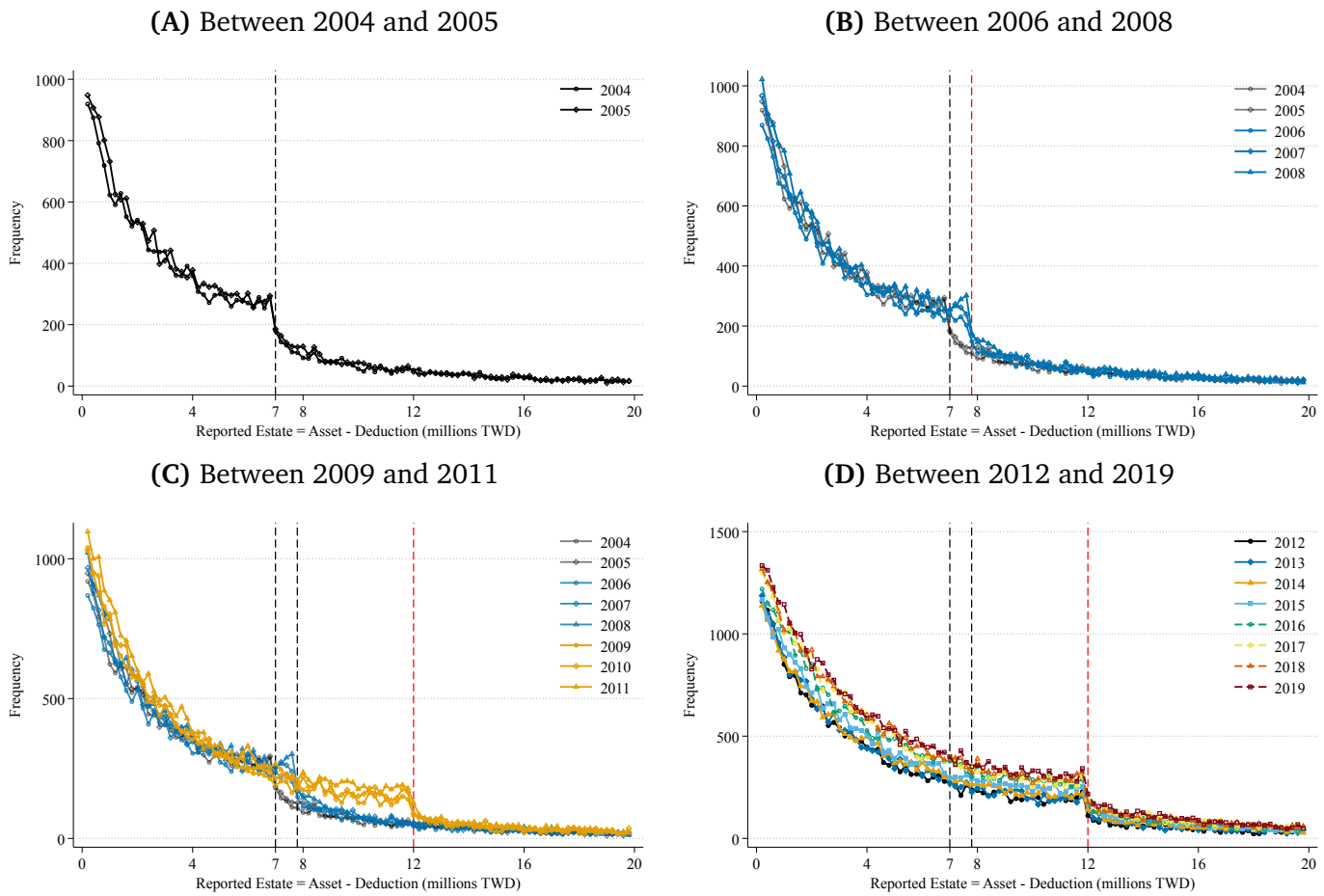
Figures

Figure 1: Estate Tax Schedule Over Time



Notes: This figure presents the estate tax schedules over time. The black line represents the schedule before 2006, with an exemption threshold of 7 million TWD (approximately 233K USD) and marginal tax rates (MTRs) ranging between 2% and 50%. The blue line plots the schedule between 2006 and 2008, where the exemption threshold increased to 7.79 million TWD, with MTRs remaining unchanged. The orange line draws the schedule between 2009 and 2016, where the exemption threshold is 12 million TWD and a flat MTR of 10%. The red dashed line is the schedule after 2017, where the exemption threshold stays the same but two new MTRs brackets are introduced for those above 62 million TWD. The corresponding percentiles for 7.79 million, 12 million, and 62 million TWD are the 96th, 98th, and 99.5th percentiles, respectively. Click here to return to the text.

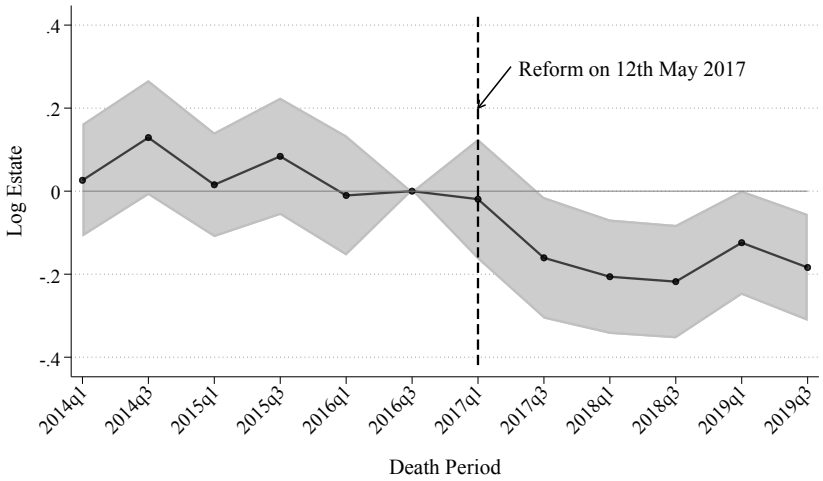
Figure 2: Density of Reported Estate Around Thresholds Over Time



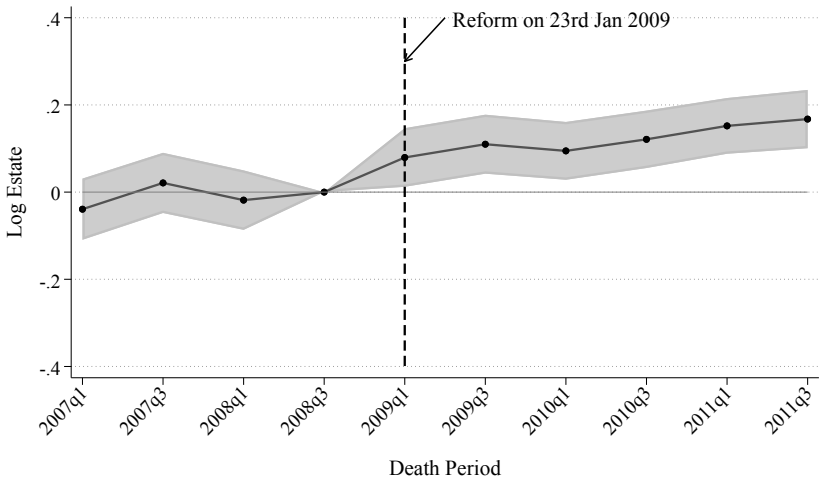
Notes: This figure presents the distribution of individuals around the exemption thresholds over time. Individuals are binned every 200K TWD (approximately 6K USD) interval. Panel A shows the distribution between 2004 and 2005 and the black dashed line corresponds to the 7 million TWD threshold. Panel B shows the distribution between 2004 and 2008. The black dashed line is the old 7 million TWD threshold and the red one is the new 7.79 million TWD threshold. Panel C shows the distribution between 2009 and 2011. The black dashed lines on the left are the old 7 and 7.79 million TWD thresholds and the red dashed line is the new 12 million TWD threshold. Panel D plots all the distribution of the later years, showing that the excess mass at the 2009-induced new threshold remains even ten years later. Click here to return to the text.

Figure 3: The Effects of Estate Tax Reforms on Reported Estate

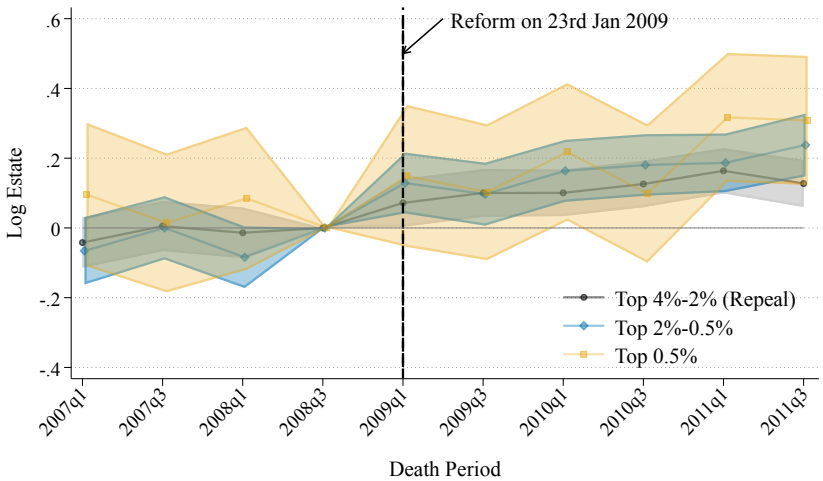
(A) 2017 Tax Increase: Top 0.5%



(B) 2009 Tax Cut: Top 4%



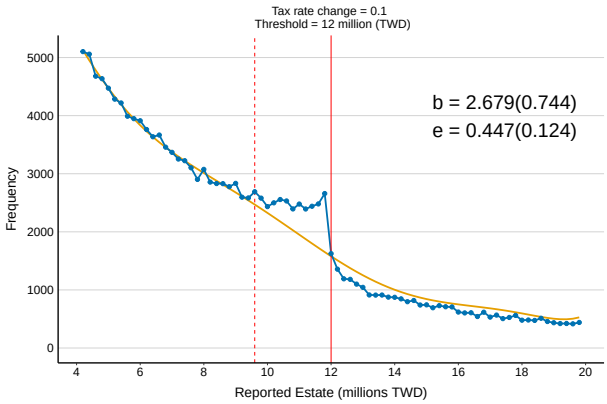
(C) 2009 Tax Cut: Splitting Top 4%-2% (Repeal), Top 2%-0.5%, Top 0.5%



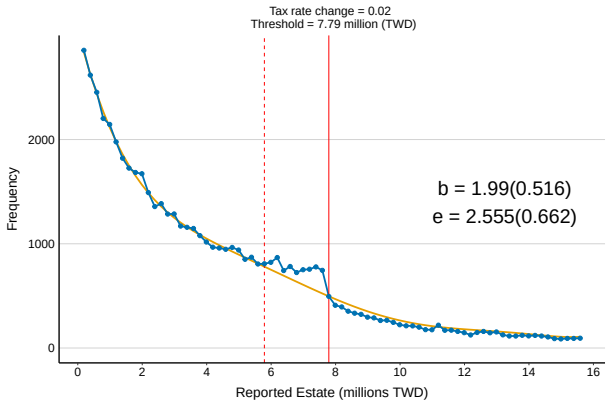
Notes: This figure presents the evolution of reported estates before and after the tax reforms. Panel A shows the result of the 2017 tax increase where the treated are those predicted to be the top 0.5% and the control are those predicted to be between the 90th and 96th percentiles. Panel B and C refers to the results of the 2009 tax cut where the reform affected the top 4%. The treated in Panel B are those predicted to be above the top 4% and the control are those predicted to be between the 90th and 96th percentiles. Panel C splits the top 4% treated group into three subgroups: top 4%-2% (repeal), top 2%-0.5%, and top 0.5%. The control remains the same. These graphs plot the estimated coefficients of the interaction term and their 95% confidence intervals from Specification (1). Click here to return to the text.

Figure 4: Bunching Estimates Using Exemption kinks v.s. Within-positive kinks

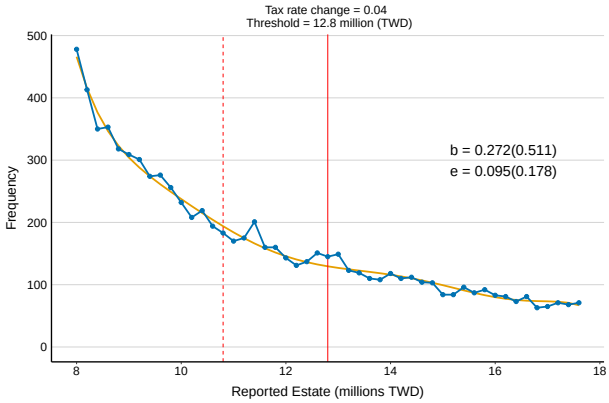
(A) Exemption: 12M TWD, 2009-2019



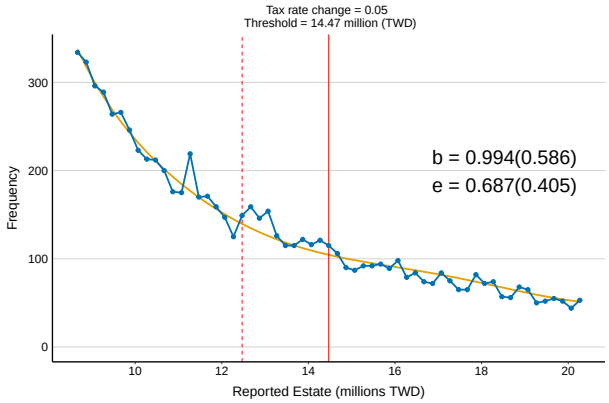
(B) Exemption: 7.79M TWD, 2006-2008



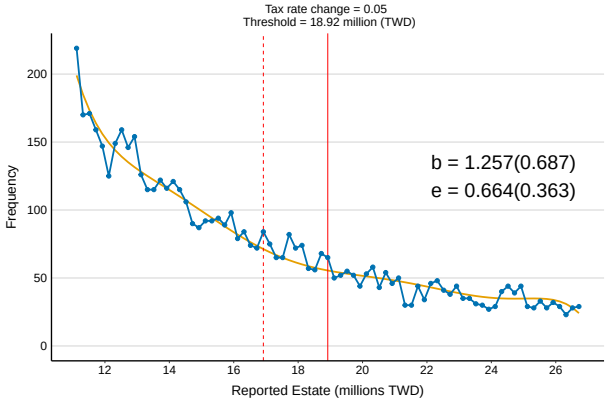
(C) Within-positive: 12.8M TWD, 2006-2008



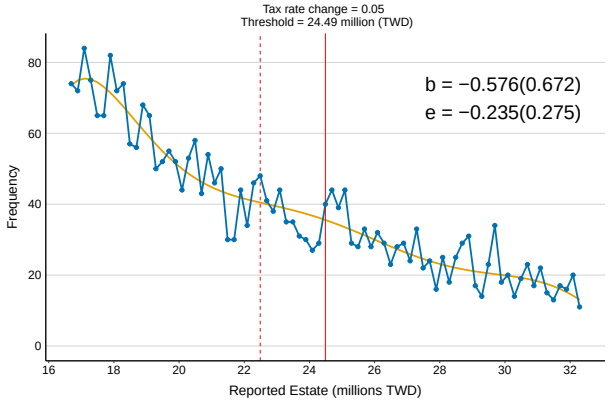
(D) Within-positive: 14.47M TWD, 2006-2008



(E) Within-positive: 18.92M TWD, 2006-2008

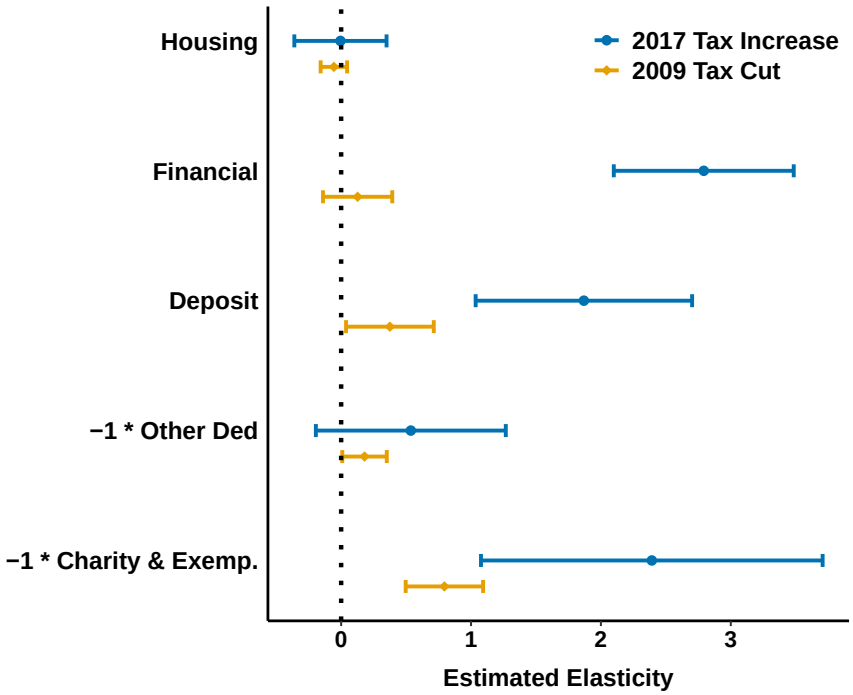


(F) Within-positive: 24.49M TWD, 2006-2008



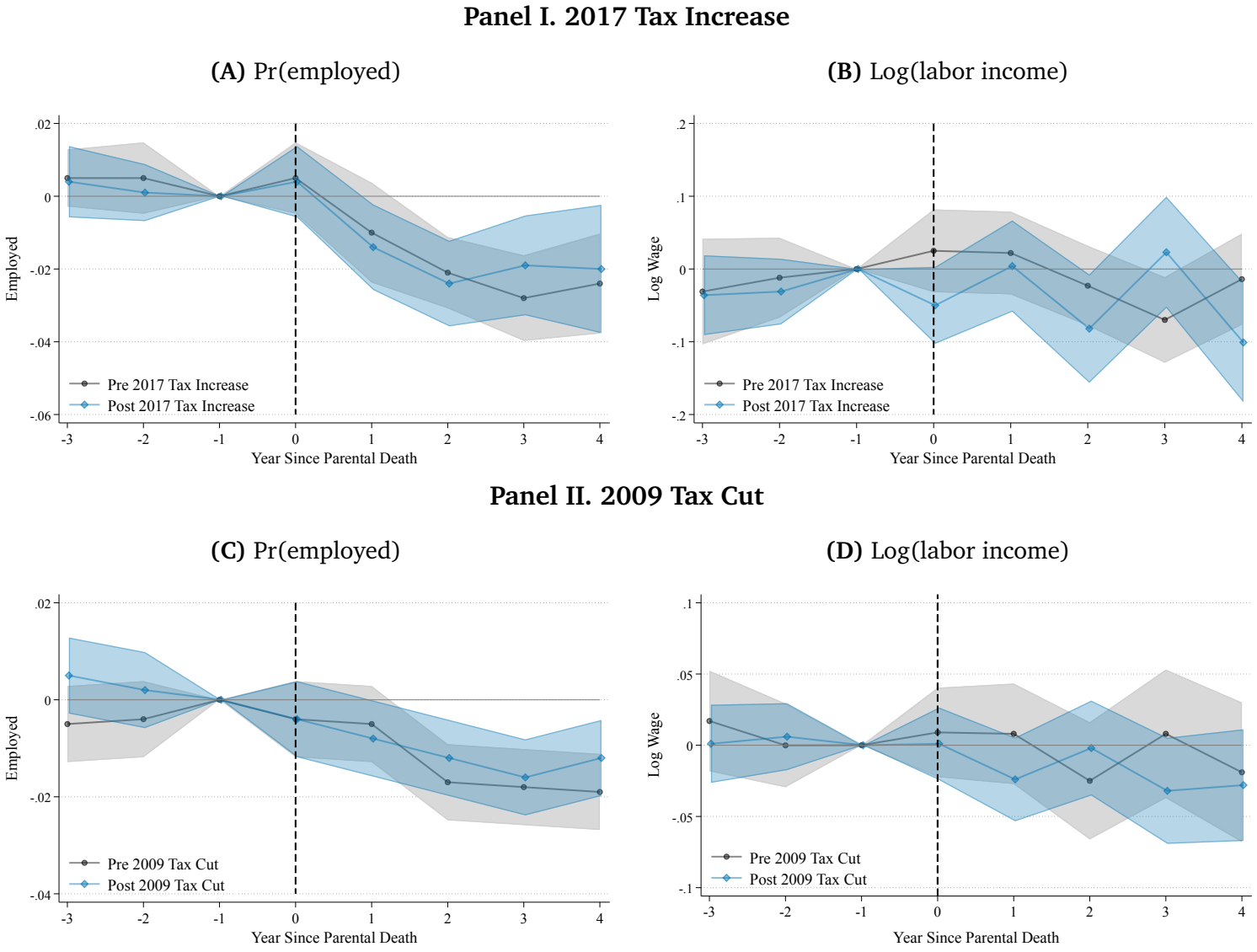
Notes: This figure groups individuals into bins of 200,000 TWD of reported estates and plots the frequency around different kinks, categorized by whether the kink is around an exemption threshold (exemption kink) or adjusts tax rates within the positive region (within-positive kink). Panel A focuses on the years between 2009 and 2019, when the exemption threshold was 12 million TWD. Panels B through F focus on the progressive schedule between 2006 and 2008, when the exemption threshold was 7.79 million TWD. Panel B shows the density around the exemption threshold, while Panels C through F present the densities around the within-positive kinks at 12.8, 14.47, 18.92, and 24.49 million TWD, respectively. Click here to return to the text.

Figure 5: Elasticity Estimates of Each Item



Notes: This figure presents elasticity estimates by estate items, categorized into housing, financial assets, deposit savings, other deductibles, and charity and exemptions. The estimates for the 2017 tax increase are shown in blue, while those for the 2009 tax cut are shown in orange. Detailed first-stage and reduced-form estimates used for the calculations are presented in Appendix Table C.1 and Table C.2. The treatment group in the estimation are individuals predicted to be above the 0.5th percentile, while the control group are those predicted to be between the 90th and 96th percentiles. Click here to return to the text.

Figure 6: The Effects of Estate Tax Reforms on Heirs' Outcomes



Notes: This figure presents the event study estimates from Specification 5. In the regressions, the treated group consists of heirs of donors predicted to be above the top 0.5%, while the control group consists of heirs whose parents are predicted to be between the 90th and 96th percentiles. Panel I shows the results for the 2017 tax increase, with outcomes on the extensive margin and intensive margin displayed in Panels A and B, respectively. Similarly, Panel II shows the results for the 2009 tax cut. Click here to return to the text.

Tables

Table 1: Diff-in-diff Estimates and Implied Elasticities

	(1)	(2)	(3)	(4)	(5)
	2017 Tax Increase	**2009 Tax Cut**			
	All	All	Repeal	Pure Decrease	
	Top 0.5%	Top 4%	Top 4%-2%	Top 2%-0.5%	Top 0.5%
First stage	-0.074	0.100	0.045	0.128	0.331
	(0.001)	(0.001)	(0.001)	(0.001)	(0.005)
	[29,576]	[25,569]	[20,918]	[19,167]	[16,634]
Reduced form	-0.204	0.131	0.130	0.204	0.154
	(0.029)	(0.016)	(0.016)	(0.021)	(0.044)
	[63,627]	[71,234]	[59,045]	[52,925]	[46,410]
Elasticity	2.757	1.310	2.889	1.594	0.465
	(0.394)	(0.161)	(0.261)	(0.166)	(0.133)

Notes: This table shows the treatment effect on log net-of-tax-rate change (first-stage) and on log reported estates (reduced form), and the implied elasticities of reported estates. Column (1) shows the result of the 2017 tax increase. Columns (2)-(5) are the results of the 2009 tax cut where (2) uses the treated group top 4%. Column (3)-(5) split the top 4% into top 4%-2% (repeal), top 2%-0.5%, and top 0.5%, respectively. Standard errors are in parentheses and the number of observations are in brackets. Click here to return to the text.

Table 2: Elasticity of Estate/Inheritance w.r.t Net-of-tax Rate in Past Literature

Paper	Variation	Context	Outcome variable	Est. elasticity w.r.t. $1-\tau$
Panel A: DiD (repeal)				
Mas-Montserrat (2019)	Repeal (top 5%-1%)	Spain	Inheritance	2.51*** (s.e. 0.33)
***Our study	Repeal (top 4%-2%)	Taiwan	Estate	2.89*** (s.e. 0.36)
Panel B: DiD (within-positive rate change)				
***Our study	Decrease (top 2%-0.5%)	Taiwan	Estate	1.59*** (s.e. 0.17)
***Our study	Decrease (top 0.5%)	Taiwan	Estate	0.47*** (s.e. 0.13)
***Our study	Increase (top 0.5%)	Taiwan	Estate	2.76*** (s.e. 0.39)
Panel C: Bunching (exemption kink)				
Escobar et al. (2019)	Bunching (0% to 10%)	Sweden	Inheritance	1.53*** (s.e. 0.10)
***Our study	Bunching (0% to 2%)	Taiwan	Estate	3.17*** (s.e. 0.51)
***Our study	Bunching (0% to 10%)	Taiwan	Estate	0.57*** (s.e. 0.10)
Panel D: Bunching (within-positive kink)				
Glogowsky (2021)	Bunching (7% to 50%)	Germany	Inheritance	0.03*** (s.e. 0.01)
Escobar et al. (2019)	Bunching (10% to 20%)	Sweden	Inheritance	0.34*** (s.e. 0.10)
Escobar et al. (2019)	Bunching (20% to 30%)	Sweden	Inheritance	0.02 (s.e. 0.10)
***Our study	Bunching (11% to 15%)	Taiwan	Estate	0.10 (s.e. 0.19)
***Our study	Bunching (15% to 20%)	Taiwan	Estate	0.27 (s.e. 0.19)
***Our study	Bunching (20% to 25%)	Taiwan	Estate	0.28 (s.e. 0.19)
***Our study	Bunching (25% to 33%)	Taiwan	Estate	0.04 (s.e. 0.26)
Panel E: RDD				
Escobar (2017)	Tax decrease	Sweden	Inheritance	0.88*** (s.e. 0.36)
Panel F: OLS				
Kopczuk and Slemrod (2001)	Time-series	U.S.	Estate	Noisy, [-0.11, 0.09]
Joulfaian (2006)	Time-series	U.S.	Tax rev/Avg tax rate	0.14*** (s.e. 0.05)

Notes: Mas-Montserrat (2019)'s number is from Table 4 Column 2. We use the version where they converted the heir-level MTR to an aggregate donor-level MTR as that is more similar to our estate tax setting. Escobar et al. (2019)'s number is from Figure C1. Glogowsky (2021)'s number is from Column 5 in Table 1. Kopczuk and Slemrod (2001)'s number is from Table 3 Column III. Joulfaian (2006) reported an estimated elasticity w.r.t. tax rate. We convert this by multiplying the number with $-(1-\tau)\tau$ and we use 0.4 for τ. Click here to return to the text.

Table 3: Elasticity of Each Item and Decomposition, Top 0.5%

	(1)	(2)	(3)	(4)	(5)	(6)
	2017 Tax Increase			**2009 Tax Cut**		
	Elasticity	Weight	Product	Elasticity	Weight	Product
Panel A: Asset						
Housing	-0.005 (0.181)	44.5%	-0.008	-0.056 (0.052)	47.6%	-0.117
Financial Asset	2.792 (0.353)	14.7%	1.367	0.127 (0.136)	10.1%	0.055
Deposit Saving	1.869 (0.425)	5.8%	0.361	0.376 (0.172)	3.8%	0.062
Subtotal Asset			1.720			0.001
Panel B: Deduction						
-1*Other Deductible	0.537 (0.373)	29.4%	0.524	0.180 (0.087)	31.6%	0.250
-1*Charity & Exemp.	2.392 (0.671)	5.6%	0.448	0.795 (0.152)	6.9%	0.240
Subtotal Deduction			0.972			-0.490
Estate (= Asset-Deduction)			2.692			0.491
% from Asset			**63.9%**			**0.3%**
% from Ded			**36.1%**			**99.7%**

Notes: This table presents the decomposition computation from the analysis in Section 4.1 that focuses on the top 0.5%. Panel A shows the asset breakdown, divided into housing, financial assets, and deposit savings, while Panel B shows the deduction breakdown, divided into other deductibles and charity and exemptions. Columns (1) through (3) display the results for the 2017 tax increase: Column (1) contains the elasticity estimates for each item, Column (2) shows each item's weight relative to the total estate, and Column (3) presents the product of the elasticity and weight. Similarly, Columns (4) through (6) present the results for the 2009 tax cut. The last two rows calculate the contributions of assets and deductions to total estates. Click here to return to the text.

Table 4: The Effect of Estate Tax Changes on Donors' Pre-Death Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
	2017 Tax Increase			**2009 Tax Cut**		
	At Death	1 Year Before	2 Years Before	At Death	1 Year Before	2 Years Before
All dependent variables are scaled by one's average pre-reform wealth						
Panel A: Housing						
Treated#Post	-0.094	-0.066	-0.027	0.143	0.188	0.120
	(0.053)	(0.060)	(0.049)	(0.087)	(0.176)	(0.099)
Panel B: Financial Asset						
Treated#Post	-0.154^{***}			0.020		
	(0.018)			(0.018)		
Panel C: Stock						
Treated#Post		-0.041^{***}	-0.039^{***}		0.012	0.016
		(0.009)	(0.009)		(0.012)	(0.012)
Panel D: Private stock						
Treated#Post		-0.023^{***}	-0.025^{***}		0.006	0.008
		(0.005)	(0.006)		(0.007)	(0.007)
Panel E: Public stock						
Treated#Post		-0.011^{*}	-0.008		0.004	0.006
		(0.005)	(0.005)		(0.008)	(0.008)
Panel F: Labor Income						
Treated#Post		-0.0002	0.0001		0.0005	0.0007
		(0.0002)	(0.0002)		(0.0003)	(0.0004)
Number of obs	63,627	63,627	63,627	46,410	46,410	46,410

Notes: This table presents the estimated coefficients of the interaction term from Specification (3). The dependent variables are all scaled by the donor's average pre-reform wealth to account for zeros. The treated and control groups are defined as previously: the treated group includes those predicted to be in the top 0.5%, while the control group includes those predicted to be between the 90th and 96th percentiles. Columns (1) to (3) display the outcomes at death, one year before death, and two years before death, respectively, for the 2017 reform. Columns (4) to (6) present the outcomes at death, one year before death, and two years before death, respectively, for the 2009 reform. Click here to return to the text.

Table 5: The Effect of Estate Tax Changes on Heirs' Inheritance (thousands TWD)

	(1) **2017 Tax Increase**	(2) **2009 Tax Cut**
Treated#PostReform	-15,164.081***	9,794.326***
	(1,920.561)	(1,262.188)
Num of heirs	72,704	53,597

Notes: This table presents the estimated coefficient on the interaction term in Specification 4, indicating the treatment effect of the reforms on inheritance. The treated group consists of heirs whose parents are predicted to be in the top 0.5%, while the control group includes those whose parents are predicted to be between the 90th and 96th percentiles. Column (1) shows the result for the 2017 tax increase, and Column (2) shows the result for the 2009 tax cut. Click here to return to the text.

Table 6: Data Moments and Results

	(1)	(2)	(3)	(4)
	2017 Tax Increase		**2009 Tax Cut**	
	Estimated Elasticity		Estimated Elasticity	
	$\hat{\varepsilon}^{TaxCut} = 0.465$	$\hat{\varepsilon}^{TaxInc} = 2.757$	$\hat{\varepsilon}^{TaxCut} = 0.465$	$\hat{\varepsilon}^{TaxInc} = 2.757$
Panel A: Parameters				
Mean τ_{t-1}	0.098	0.098	0.340	0.340
Mean $d\tau$	0.064	0.064	-0.250	-0.250
Aggregate RE_{t-1}	175125.783	175125.783	35000.610	35000.610
Panel B: Tax Revenues				
Mechanical (dM)	11208.050	11208.050	-16474.388	-16474.388
Behavioral (dB)	-566.243	-3357.271	3946.365	23482.991
$-dB/dM$	0.051	0.300	0.240	1.425
Total ($dM + dB$)	10641.807	7850.779	-12528.022	7008.603
Panel C: Welfare				
dW	-1783.968	-4574.997	12433.170	31884.929

Notes: All values in millions 2016 TWD except for tax rates or ratios. τ^{t-1} is the tax rate in 2016 for the 2017 tax increase and in 2008 for the 2009 tax cut. Aggregate RE_{t-1} is the total sum of reported estates of the top 0.5% in 2016 for the 2017 tax increase and in 2008 for the 2009 tax cut. Click here to return to the text.

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A Appendix to Section 2: Institutional Context & Data

A.1 Tax Assessment

Table A.1: Details of the Breakdown of Estates

Item	Description
Panel A: Assets	
Housing	Land and houses are third-party-reported where the government could verify from the property tax data. Housing values are determined by government-announced prices, which are typically updated biannually and lower than the market prices.
Financial Assets	Include publicly-listed and privately-listed stock, and other financial instruments. Stock ownership is third-party-reported which the government can verify from the dividend income tax records. Publicly-listed stock is assessed at the market price on the death date. Privately-listed stock is valued at the net assets of the company on the death date. Other financial assets are self-reported items such as trusts, insurance, farm output, credits, cash, and jewelry.
Deposit savings	Domestic bank savings are self-reported, although the government has a third-party-reported proxy interest income from the individual income tax records.
Panel B: Deductions	
Charitable/Exemptions	Charitable contributions are donations to nonprofit organizations. Exemptions include spousal claims and estate taxes paid within the last five years. Spousal claims are based on Article 1030-1 of Taiwanese Civil Law: "Upon dissolution of the statutory marital property regime, the remainder of the property acquired by the husband or wife during marriage, after deducting the debts incurred during the continuance of the marriage relationship, if any, shall be equally distributed between the husband and the wife." If the surviving spouse owns less wealth than the decedent, they may claim half of the decedent's wealth before it is counted as part of the estate.

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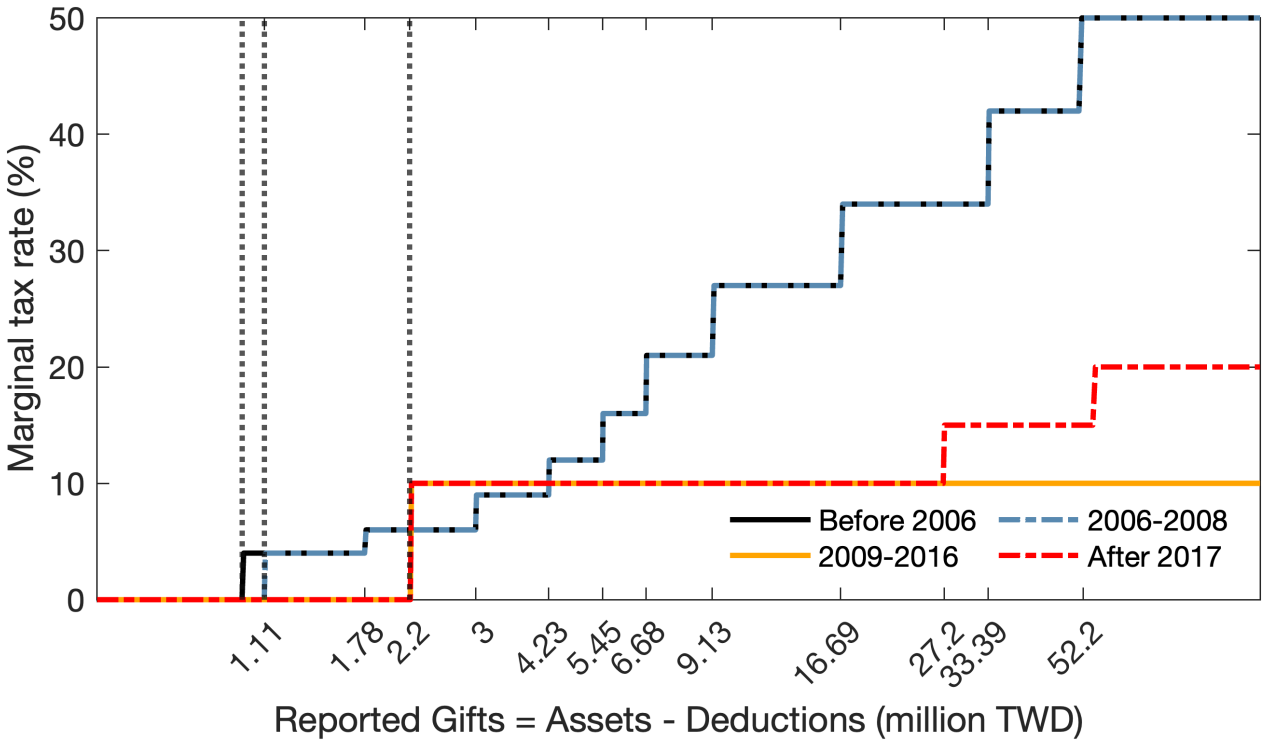
Table A.1: (Continued)

Item	Description
Other Deductibles	These include debts, farm, funeral, parent, dependent, spousal, and disability deductions. Debt deductions require submission of relevant loan statements. Farm deductions represent the value of agriculturally-used properties. To claim a farm deduction, a certificate from the government must be provided, verifying that the land is used for agricultural purposes. Dependent deductions were 450,000 TWD per dependent before 2009, increasing to 500,000 TWD after 2014. Parent deductions were 1 million TWD per parent before 2009 and 1.23 million TWD after 2014. Spousal deductions were 4.45 million TWD before 2009, increasing to 4.93 million TWD after 2014. Disability deductions were 5.57 million TWD before 2009, increasing to 6.18 million TWD after 2014. Funeral deductions were 1.11 million TWD before 2009 and 1.23 million TWD after 2009.

Notes: Click here to return to the text.

A.2 Gift Tax Schedule

Figure A.1: Gift Tax Schedule Over Time



Notes: This figure presents the gift tax schedules over time. The black line represents the schedule before 2006, with an exemption threshold of 1 million TWD and marginal tax rates (MTRs) ranging between 4% and 50%. The blue line plots the schedule between 2006 and 2008, where the exemption threshold increased to 1.11 million TWD, with MTRs remaining unchanged. The orange line draws the schedule between 2009 and 2016, where the exemption threshold is 2.2 million TWD and a flat MTR of 10%. The red dashed line is the schedule after 2017, where the exemption threshold stays the same but two new MTRs brackets are introduced for those above 27.2 million TWD. Click here to return to the text.

B Appendix to Section 3: Empirical Analysis

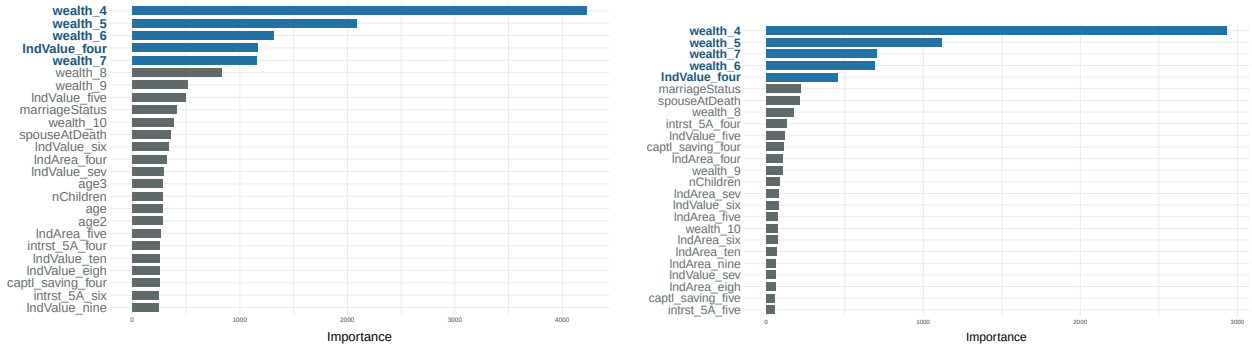
B.1 2017 Prediction Algorithm Details

Procedures. To construct donors' counterfactual estates in the absence of the reform, we use a random-forest algorithm to predict a donor's reported estates at death based on her past wealth and demographic information. The procedure is as follows:

- Within those who died before the reform (2014-2016), select 70% as a training sample and the remaining 30% as a testing sample
- Train a random forest model where the outcome variable is a discrete group variable indicating the classification of one's percentile of estate: i) below P90, ii) P90-P96, iii) P96-P99.5, iv) above P99.5
- Predictors are the values and quantities of land, houses, stocks, capitalized interest income, earned income, unearned income, whether a business owner [4 years, 5 years,.., 10 years before death], and gender, age, number of kids, marriage status, born place, parents alive etc
- In the above step, we tune parameters for the number of trees, number of variables possibly split at each node, minimal node size
- Repeat the above step for each set of parameters and calculate the corresponding precision and recall rates for each set of parameters
- Choose the best parameter set
- Apply it to the held-out testing sample in the beginning to compute the precision and recall rates
- In the final step, we apply the trained algorithm to ALL death samples in our studied period (2014-2019)
- As we observe everyone's pre-reform predetermined wealth and demographic variables, which serve as the predictors in the algorithm, we can back out everyone's counterfactual estate percentile group had the reform not happened

Important Predictors. We report the most important top 25 predictors in our algorithm. Panel A in Figure B.2 presents the overall top 25 most important predictors in our algorithm (2014-2016). Panel B shows those when using 2014-2015. In both periods, the top 5 are the same: wealth four years before death, wealth five years before death, wealth 6 years before death, wealth 7 years before death, and land values four years before death, with just the difference of the top 3-5 being reorderd.

Figure B.2: Top 25 Important Predictors



(A) Overall: 2014-2016 **(B)** 2014-2015

Performance. Table B.2 presents the prediction performance, measured in precision and recall rates using the held-out samples. Precision rate is the number of individuals being correctly predicted in the group over the number of individuals being predicted in the group. Recall rate is the number of individuals being correctly predicted in the group over the number of individuals actually in the group.

Column (1) and (2) refer to the precision and recall rates of the overall performance of the 2017 prediction algorithm, which uses 2014-2016 as the training samples. Then, to show the stability over time, in Column (3) to (8) we split them into 2014, 2015, and 2016, respectively.

Overall, the precision rates range from 61% to 82% by group, and the recall rates range from 60% to 88%. When splitting them by year, the results are similar, landing support on the stability of the algorithm.

Table B.2: Performance of 2017 Prediction Algorithm

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Overall		2014		2015		2016	
Group	Precision	Recall	Precision	Recall	Precision	Recall	Precision	Recall
P90	81.8%	87.8%	78.5%	88.1%	82.4%	88.1%	84.0%	87.5%
P90-P96	60.5%	55.3%	60.1%	54.2%	60.7%	56.1%	60.7%	55.5%
P96-P99.5	66.3%	62.1%	67.6%	57.8%	67.3%	62.0%	64.4%	66.3%
P99.5	76.0%	59.5%	78.5%	56.7%	75.3%	63.0%	74.8%	59.0%

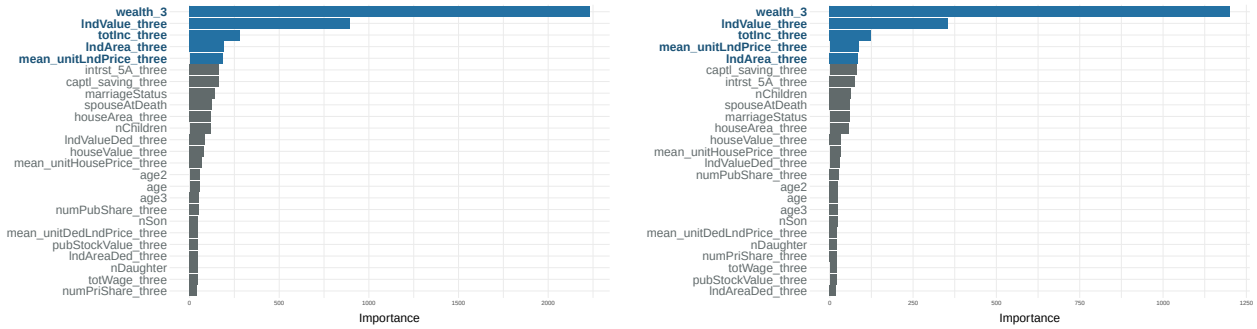
B.2 2009 Prediction Algorithm Details

Procedures. To construct donors' counterfactual estates in the absence of the reform, we use a random-forest algorithm to predict a donor's reported estates at death based on her past wealth and demographic information. The procedure is as follows:

- Within those who died before the reform (2007-2008), select 70% as a training sample and the remaining 30% as a testing sample
- Train a random forest model where the outcome variable is a discrete variable indicating the group of the individual: i) below P90, ii) P90-P96, iii) P96-P8, iv) P98-P99.5, v) above P99.5
- Predictors are the values and quantities of land, houses, stocks, capitalized interest income, cars, earned income, unearned income, whether a business owner [3 years before death] and gender, age, number of kids, marriage status, born place, parents alive etc
- In the above step, tune parameters for the number of trees, number of variables possibly split at each node, minimal node size
- Repeat the above step for each set of parameters and calculate the corresponding precision and recall rates for each set of parameters. Choose the best parameter set
- Apply it to the held-out testing sample in the beginning and compute the precision and recall rates
- In the final step, we apply the trained algorithm to ALL death samples in our studied period (2007-2011)

Important Predictors. We report the most important top 25 predictors in our algorithm. Panel A in Figure B.3 presents the overall top 25 most important predictors in our algorithm (207-2008). Panel B shows those when using 2008 only. In both periods, the top 5 are the same: wealth three years before death, land values three years before death, land value three years before death, land area three years before death, and average land prices per squared meter three years before death.

Figure B.3: Top 25 Important Predictors



(A) Overall: 2007-2008 **(B)** 2007

Performance. We report the performance of the prediction algorithm of the 2009 reform where the 2006-2007 years are used as the training samples.

Table B.3: Performance of 2009 Prediction Algorithm

	(1)	(2)	(3)	(4)	(5)	(6)
	Overall		2007		2008	
Group	Precision	Recall	Precision	Recall	Precision	Recall
P90	70.9%	75.8%	69.7%	75.2%	71.9%	76.4%
P90-P96	60.3%	59.4%	60.8%	58.8%	60.0%	60.0%
P96-P99.5	63.1%	59.2%	62.2%	59.4%	64.0%	59.1%
P99.5	67.9%	52.7%	61.8%	48.1%	73.1%	56.6%

B.3 Robustness check: Alternative Control Groups

Table B.4: Elasticity Estimates Using Alternative Control Groups

	2017 Tax	2009 Tax Cut	
	Top 0.5%	Top 4%	Top 0.5%
P90-P93	2.991	1.441	0.507
	(0.536)	(0.238)	(0.211)
P93-P96	3.277	0.266	0.131
	(0.534)	(0.179)	(0.142)
P96-P98	2.773		
	(0.531)		
P98-P99.5	0.688		
	(0.550)		
P90-96 (baseline)	**2.745**	**1.317**	**0.465**
	(0.441)	**(0.204)**	**(0.203)**

B.4 Robustness check: Alternative Placebo Reform Year

To test the robustness of our prediction method, we use the years 2012, 2013, 2014, and 2015 as placebo reform years and show that no effect is detected under these scenarios. The procedure is as follows. In the case of the placebo reform year 2012, we take those who died between 2009 and 2011 and train a random-forest algorithm as previously described to categorize their estate groups. We then apply the trained algorithm to the death samples between 2009 and 2013 so that every dead person has a counterfactual estate groups that they belong to. We define those who are above the top 0.5% percentile as the treated and those between the P90-P96 percentiles as the control as before.

We estimate Equation 3 and present the estimates in Table B.5. In all three scenarios, the estimated coefficients are statistically insignificant, indicating that no effect on reported estate is detected.

- Take a given period and assume a placebo reform year
- Define treatment using the pre-period
- Train an algorithm and apply to all periods

Table B.5: Treatment Effect of Placebo Tests

Period	2009-2013	2010-2014	2011-2015	2012-2016
Placebo Reform	2012	2013	2014	2015
Treated#Post	-0.0548	-0.0463	0.0227	-0.0265
	(0.0361)	(0.0404)	(0.0398)	(0.0321)
Num of obs	44,901	42,701	45,569	46,322

Notes: Click here to return to the text.

B.5 Robustness check: Anticipation

Table B.6 presents the estimated elasticities assuming that there is an anticipatory behavior from donors. We..

- Define reform year 2016 (or 2008)
- Retrain the algorithm and re-predict
- Estimate diff-in-diff redefining the post dummy

Table B.6: Elasticity Estimates with Anticipatory Behavior

	2017 Tax Increase		**2009 Tax Cut**	
	Baseline	Anticipated	Baseline	Anticipated
First stage	-0.074	-0.067	0.331	0.332
	(0.001)	(0.001)	(0.005)	(0.005)
Reduced form	-0.204	-0.166	0.154	0.190
	(0.029)	(0.036)	(0.044)	(0.048)
Elasticity	2.757	2.478	0.465	0.572
	(0.394)	(0.539)	(0.133)	(0.145)

Notes: Click here to return to the text.

B.6 Robustness check: Alternative Specification for Elasticities

Average tax rates. Instead of using marginal tax rates in the first stage to compute the elasticity of reported estates with respect to the net-of-marginal-tax rate, we use average tax rates to estimate the elasticity of reported estates with respect to the net-of-average-tax rate. We estimate the same first-stage and reduced-form specifications as in Equations 2 and 3, but replace τ_i with the average tax rate.

Table B.7: Diff-in-diff Estimates and Implied Elasticities Using Average Tax Rates

	(1)	(2)	(3)	(4)	(5)
	2017 Tax Increase	**2009 Tax Cut**			
	All	All	Repeal	Pure Decrease	
	Top 0.5%	Top 4%	Top 4%-2%	Top 2%-0.5%	Top 0.5%
First stage	-0.035	0.046	0.015	0.045	0.211
	(0.001)	(0.001)	(0.001)	(0.001)	(0.004)
	[29,576]	[25,569]	[20,918]	[19,167]	[16,634]
Reduced form	-0.204	0.131	0.130	0.204	0.154
	(0.029)	(0.016)	(0.016)	(0.021)	(0.044)
	[63,627]	[71,234]	[59,045]	[52,925]	[46,410]
Elasticity	5.829	2.848	8.667	4.533	0.730
(0.845)	(0.353)	(1.213)	(0.477)	(0.209)	

Notes: This table shows the treatment effect on log net-of-average-tax-rate change (first-stage) and on log reported estates (reduced form), and the implied elasticities of reported estates. Column (1) shows the result of the 2017 tax increase. Columns (2)-(5) are the results of the 2009 tax cut where (2) uses the treated group top 4%. Column (3)-(5) split the top 4% into top 4%-2% (repeal), top 2%-0.5%, and top 0.5%, respectively. Standard errors are in parentheses and the number of observations are in brackets. Click here to return to the text.

IV-2SLS. Instead of using only the pre-period donor samples to run the first-stage as in Sepcificaiton X. We estimate the following IV-2SLS regressions:

$$\log(1-\tau_i) = \gamma_0 Treated_i \times Post_i + \gamma_1 Treated_i + \gamma_2 Post_i + \mu_i$$
$$\log Estate_i = \varepsilon \widehat{\log(1-\tau_i)} + \alpha_0 Treated_i + \alpha_1 Post_i + v_i$$

where τ_i is the marginal tax rate faced by donor i. $Treated_i$ is 1 if i' estate is predicted to be above the top 0.5% and 0 if between the 90th and 96th percentiles. $Post_i$ is 1 if i died after the reform. We use $Treated_i \times Post_i$ to instrument the net-of-tax rate change. Our parameter of interest is ε, which indicates the elasticity of reported estates with respect to net-of-tax rate.

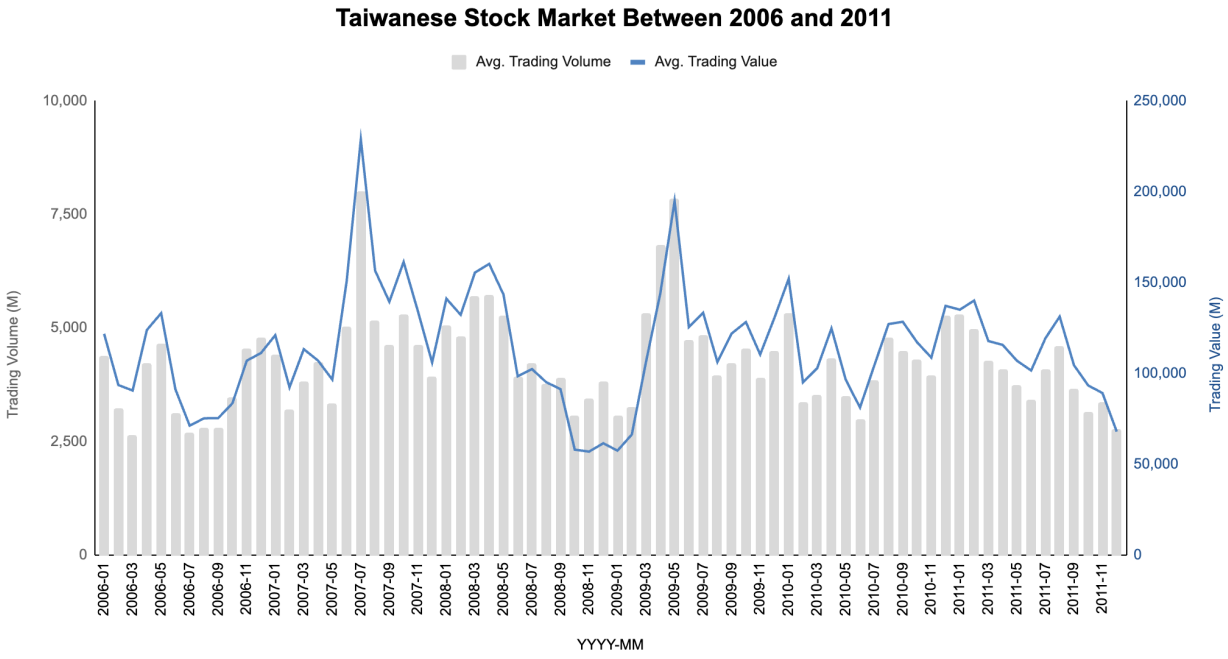
Table B.8: IV-2SLS Elasticity Estimates

	(1)	(2)	(3)	(4)
	2017 Tax Increase	**2009 Tax Cut**		
	All	All	Pure Decrease	
	Top 0.5%	Top 4%	Top 4%-0.5%	Top 0.5%
First stage	-0.043	0.097	0.069	0.328
	(0.001)	(0.001)	(0.001)	(0.002)
F test	2070.18	6542.75	4521.91	38606.29
Elasticity	4.712	1.357	2.007	0.468
	(0.543)	(0.168)	(0.232)	(0.116)
Num of obs	63,627	71,234	68,397	46,410

Notes: Click here to return to the text.

B.7 Robustness: Macro Economic Condition

Figure B.4: Taiwanese Stock Exchange Trading Volume and Values Between 2006 and 2011



Notes: This figure presents the trading volume and values in the Taiwanese Stock Exchange between 2006 and 2011. The source is https://www.twse.com.tw/en/.

Table B.9: Elasticity Estimates Split by Pre-reform Fraction of Stockholding, Top 0.5%

	Low Stock	High Stock
First stage	0.312	0.376
	(0.006)	(0.008)
	[13,426]	[3,208]
Reduced form	0.142	0.171
	(0.050)	(0.088)
	[36,937]	[9,473]
Implied Elasticity	0.455	0.455
	(0.160)	(0.234)

Notes: Click here to return to the text.

B.8 Robustness check: Gifting

Table B.10: The Effect of Reforms on Gifts Within Two Years Before Death

	(1) 2017 Tax Increase	(2) 2009 Tax Cut
Treated#Post	-0.0267***	-0.0002
	(0.0073)	(0.0073)

Notes: Click here to return to the text.

Table B.11: Predictions of Gifting Behavior When Estate and Gift Taxes Change

	Expect to Die Within Two Years (nonsudden deaths)	**Expect to Live Within Two Years (sudden deaths)**
Panel A: 2017 Both Taxes Increase		
Estate vs. Gifting	Gifting does not reduce estate ⇒ **No change**	Gifting can reduce estate, but gifting is now more costly ⇒ **Ambiguous**. Could gift more or less
Gifting vs. Other Avoidance	Want to switch gifting to other avoidance due to higher cost of gifting ⇒ **Gift less**	Want to switch gifting to other avoidance as gifting is now more costly ⇒ **Gift less**
Overall Prediction	**Gift less**	**Ambiguous**
Panel B: 2009 Both Taxes Decrease		
Estate vs. Gifting	Gifting does not reduce estate ⇒ **No change**	Gifting reduces estate, but estate is now cheaper ⇒ **Ambiguous**. Could gift more or less
Gifting vs. Other Avoidance	Will not want to switch from other avoidance to gifting as it will count toward tax base ⇒ **No change**	May switch other avoidance to gifting due to reduced cost of gifting ⇒ **Gift more**
Overall Prediction	**No change**	**Ambiguous**

Notes: Click here to return to the text.

Table B.12: The Effect of Reforms on Gifts Within Two Years Before Death

	(1)	(2)	(3)	(4)
	2017 Tax Increase		2009 Tax Cut	
	Non-sudden	Sudden	Non-sudden	Sudden
Treated#Post	-0.0264***	-0.0720	-0.0001	0.0404*
	(0.0073)	(0.0513)	(0.0073)	(0.0191)

Notes: Click here to return to the text.

C Appendix to Section 4: Mechanism

C.1 Source of Responses: Elasticity Estimates Details by Item

Table C.1: Elasticity by Items Computation, Top 0.5% 2017 Tax Increase

	(1)	(2)	(3)	(4)	(5)
	2017 Tax Increase				
	Housing	Financial	Deposit	OthDed	Charit/Exemp
Panel A: First stage (Dependent variable: $\log(1-\tau)$)					
Treated#Post	-0.074	-0.074	-0.074	-0.074	-0.074
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Panel B: Reduced form (Dependent variable: item values)					
Treated#Post	32.262	-4256.752	-1124.073	1108.972	1329.51
	(973.312)	(535.961)	(254.957)	(770.42)	(372.742)
Pre-period mean	72698.56	20603.28	8130.71	27942.14	7512.00
Rel. change	0.0005	-0.2066	-0.1383	0.0397	0.1770
	(0.0134)	(0.0260)	(0.0314)	(0.0276)	(0.0496)
Panel C: Implied Elasticity					
	-0.0054	2.7919	1.8689	-0.5365	-2.3919
	(0.1811)	(0.3534)	(0.4251)	(0.3730)	(0.6710)

Notes: Click here to return to the text.

Table C.2: Elasticity by Items Computation, Top 0.5% 2009 Tax Cut

	(1)	(2)	(3)	(4)	(5)
			2009 Tax Cut		
	Housing	Financial	Deposit	OthDed	Charit/Exemp
Panel A: First stage (Dependent variable: $\log(1-\tau)$**)**					
Treated#Post	0.331	0.331	0.331	0.331	0.331
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Panel B: Reduced form (Dependent variable: item values)					
Treated#Post	-1133.037	480.778	551.258	-1892.939	-1967.525
	(1049.019)	(480.778)	(551.258)	(915.876)	(375.244)
Pre-period mean	61155.57	11447.78	4434.14	31734.51	7479.61
Rel. change	-0.0185	0.0420	0.1243	-0.0596	-0.2631
	(0.0172)	(0.0450)	(0.0570)	(0.0289)	(0.0502)
Panel C: Implied Elasticity					
	-0.056	0.127	0.376	-0.180	-0.795
	(0.052)	(0.136)	(0.172)	(0.087)	(0.152)

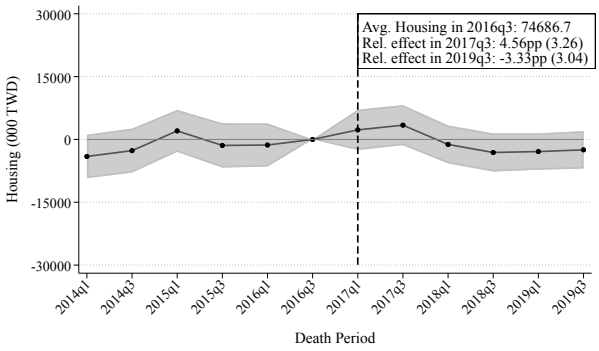
Notes: Click here to return to the text.

C.2 Sources of Responses: Event Study Graphs by Items

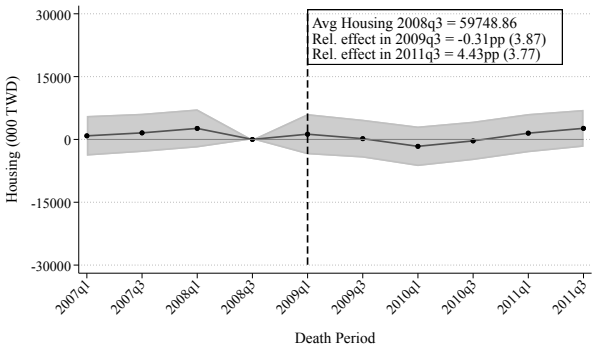
Figure C.1: Margins of Adjustment in Asset (Treated = Top 0.5% Donors)

I. Housing

(A) 2017 Tax Increase

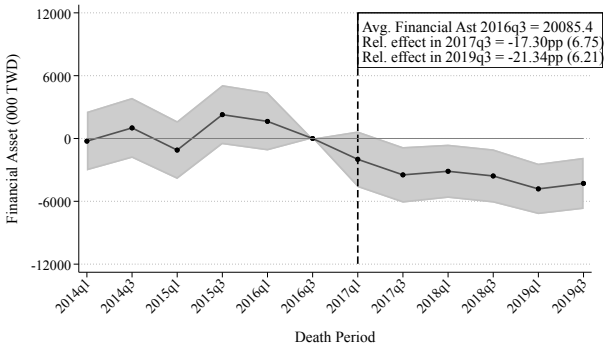


(B) 2009 Tax Cut

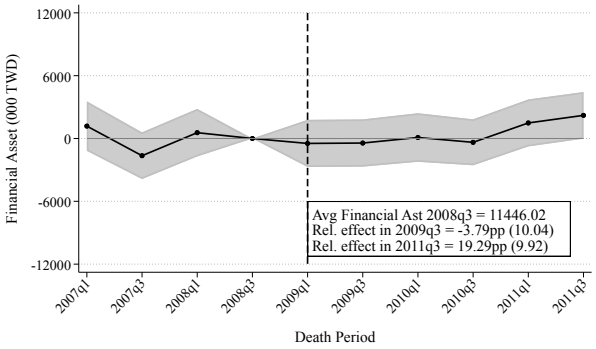


II. Financial Assets & Others

(C) 2017 Tax Increase

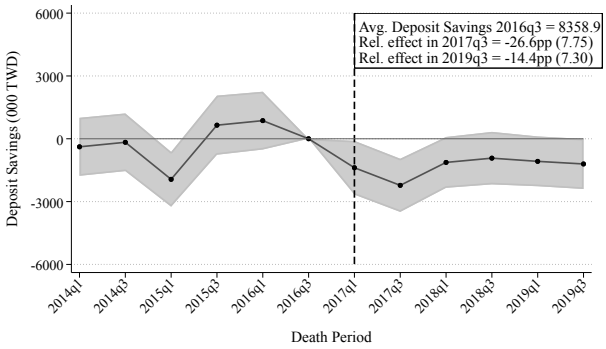


(D) 2009 Tax Cut



III. Deposit Savings

(E) 2017 Tax Increase



(F) 2009 Tax Cut

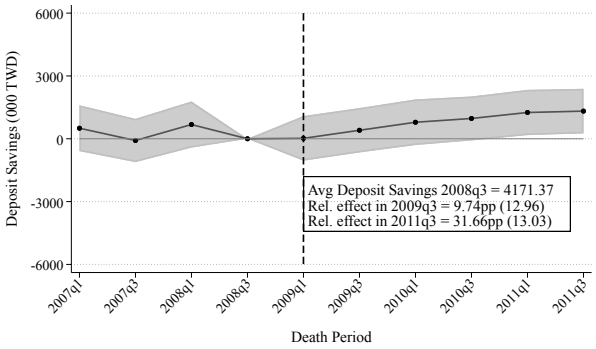
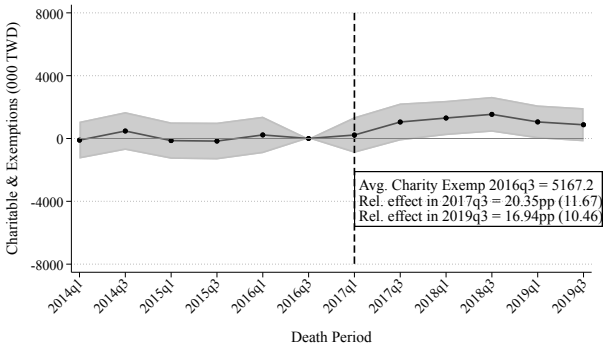


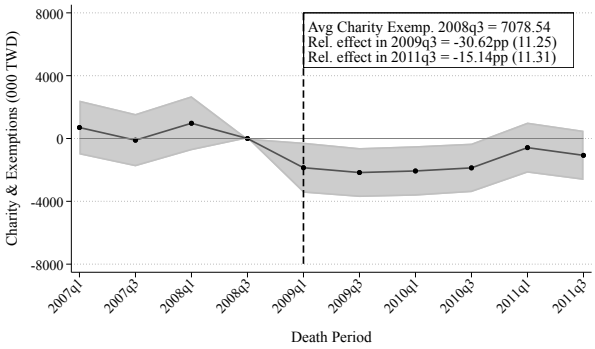
Figure C.2: Margins of Adjustment in Deduction (Treated = Top 0.5% Donors)

I. Charitable & Exemptions

(A) 2017 Tax Increase

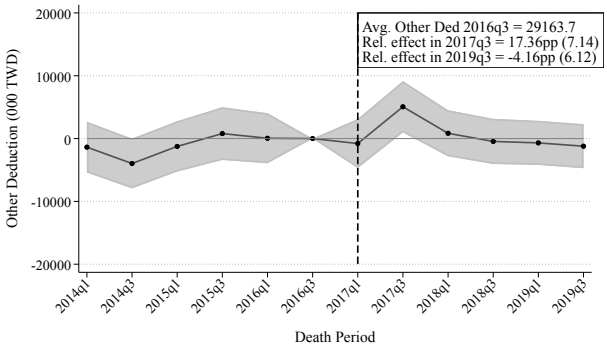


(B) 2009 Tax Cut

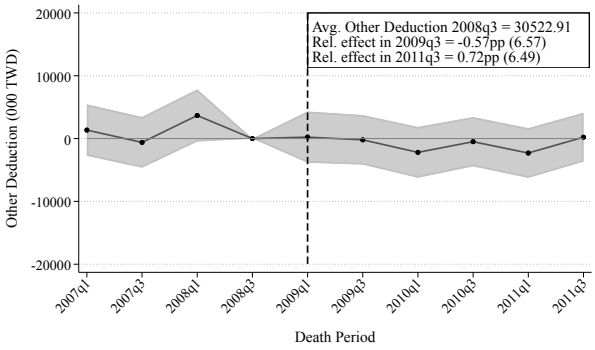


II. Other Deductions

(C) 2017 Tax Increase

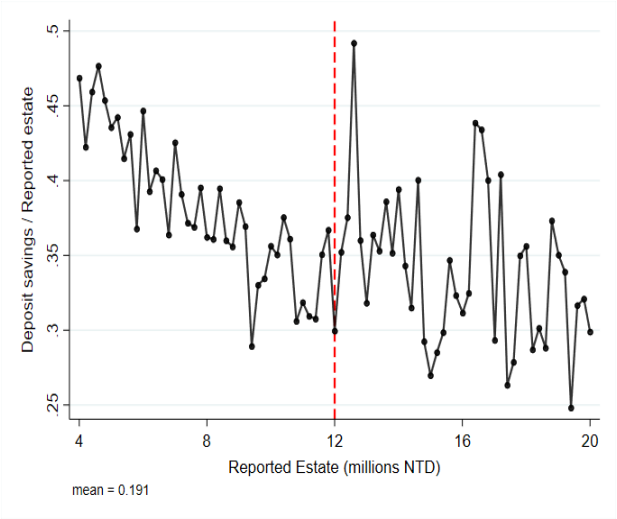


(D) 2009 Tax Cut

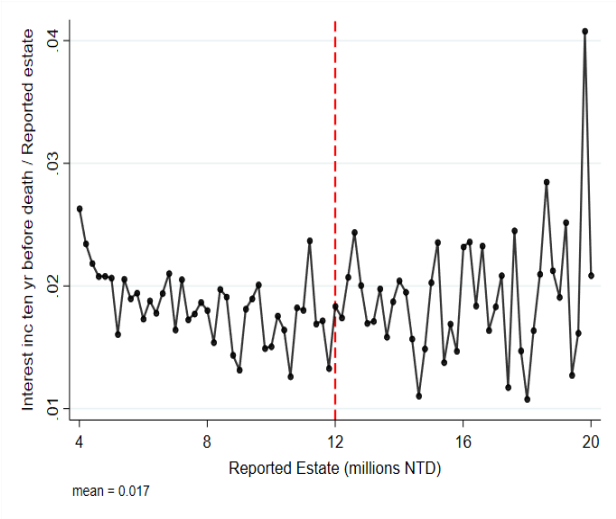


C.3 Source of Responses: Discontinuity at Reported Deposit Savings

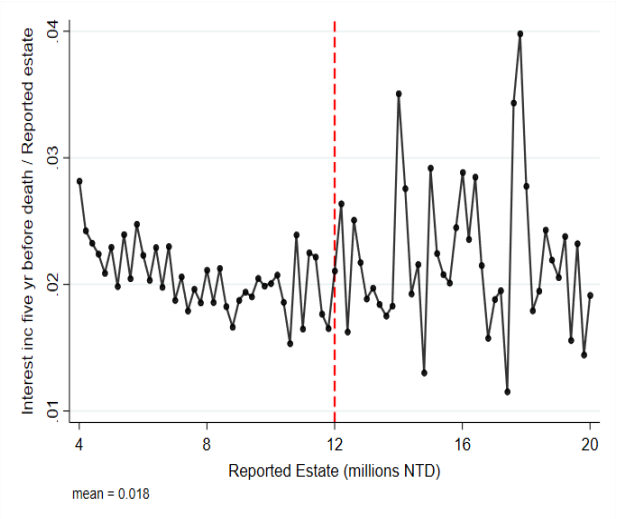
Figure C.3: Bunchers Reported Savings At Death and Interest Income Before Death (Below Death Age 60)



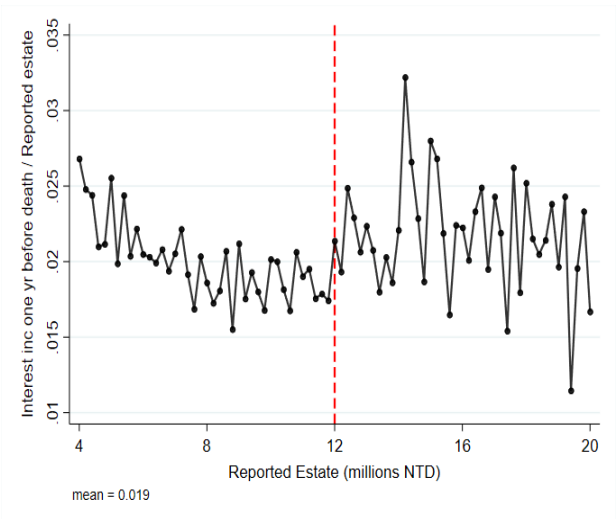
(A) Savings Reported At Death



(B) Interest Income One Year Before Death



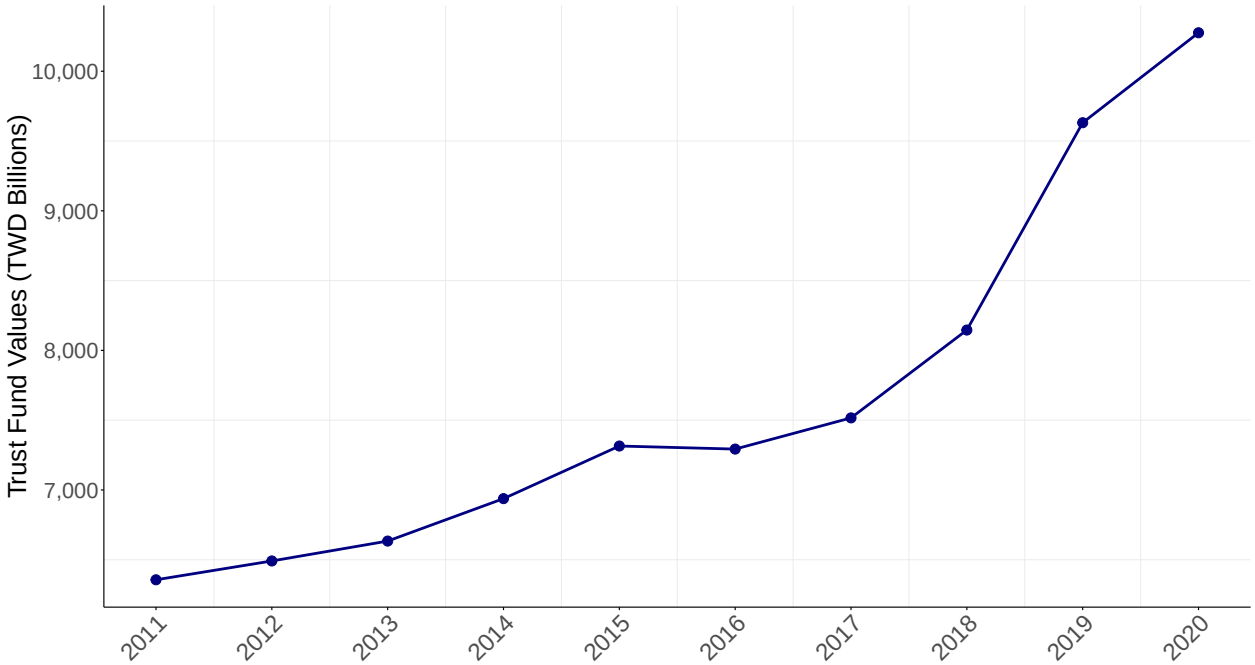
(C) Interest Income Five Years Before Death



(D) Interest Income Ten Years Before Death

C.4 Aggregate Trust Fund Sizes Over Time

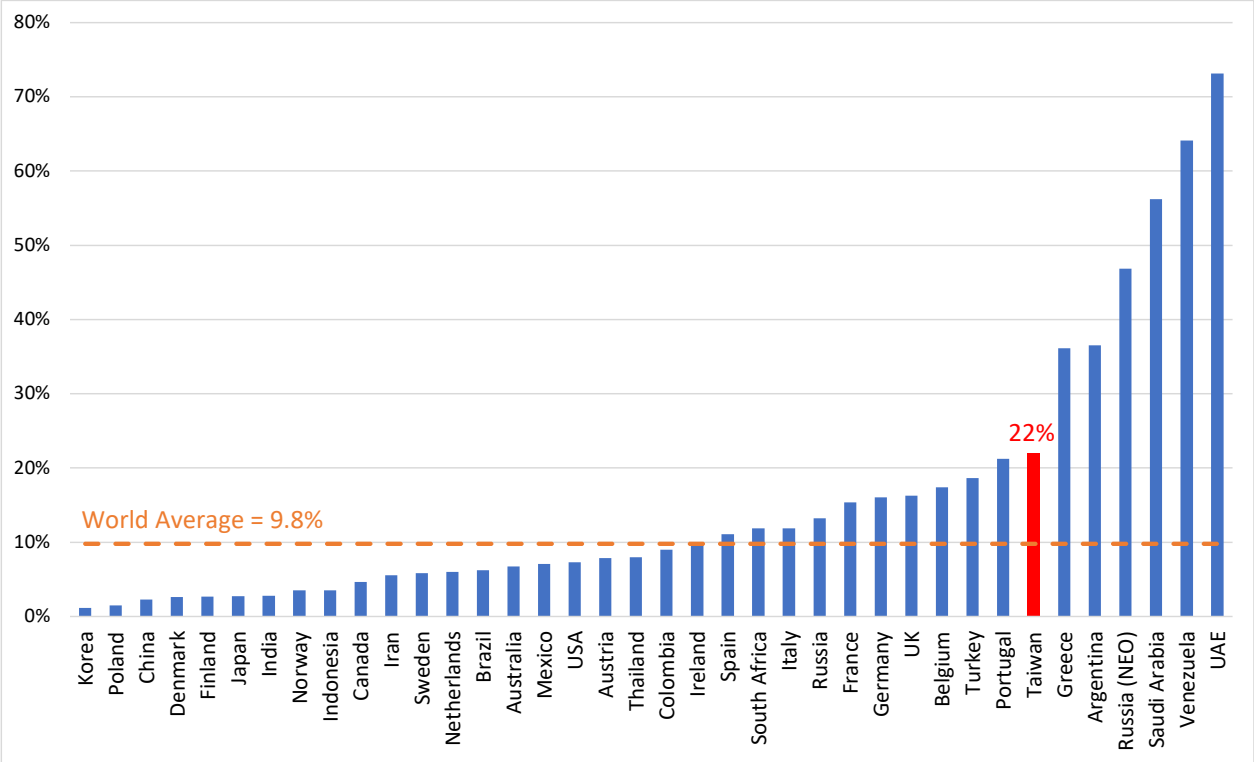
Figure C.4: Trust Fund Sizes in Taiwan Over Time



Notes: Data source: https://www.trust.org.tw/tw/about/annual-reports. Click here to return to the text.

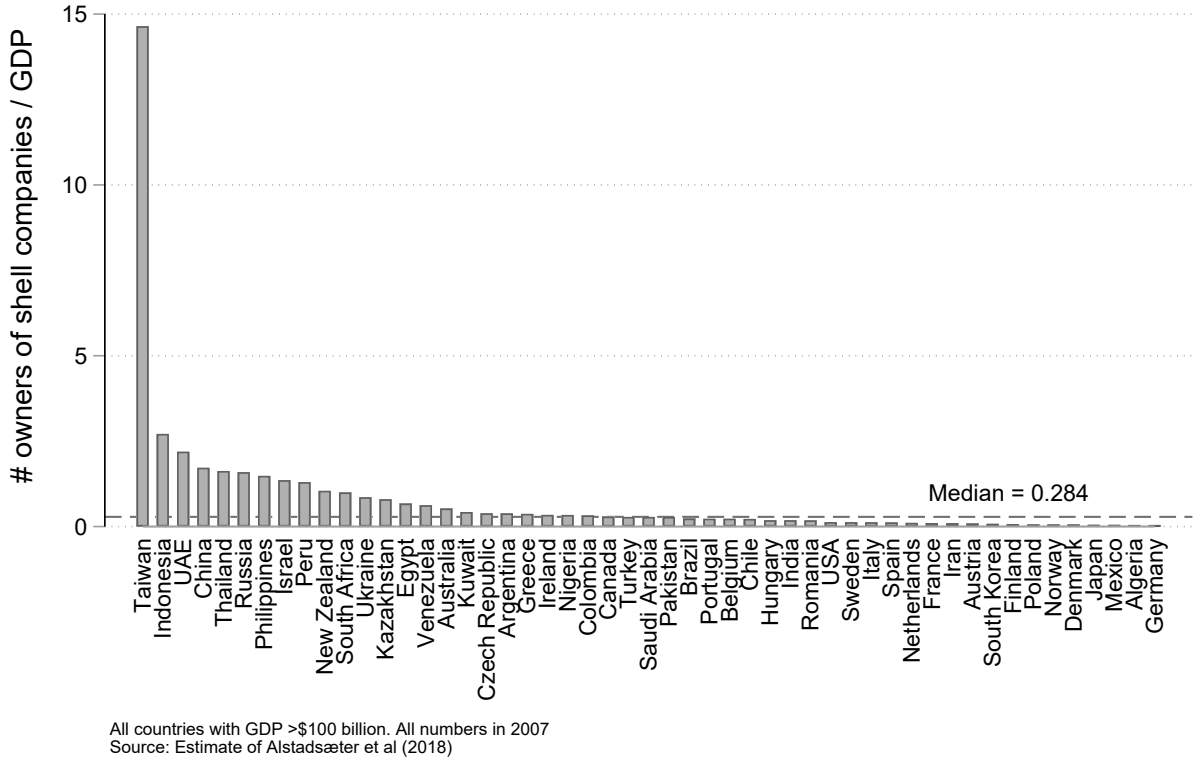
C.5 Taiwanese Fraction of Offshore Wealth

Figure C.5: Taiwanese Fraction of Offshore Wealth



Notes: Data source Alstadsæter et al. (2018). Click here to return to the text.

Figure C.6: Taiwanese Fraction of Offshore Wealth



Notes: Data source Alstadsæter et al. (2018). Click here to return to the text.

Family Businesses, Nepotism, and Productivity*

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Abstract

This paper provides the first systematic evidence on kinship networks within and across businesses by linking comprehensive administrative data on family ties, firm ownership, firm-to-firm transactions, and employer-employee records for the universe of firms in Taiwan. The unique dataset enables us to document the importance of family connections both within and across firms. We find that 40% of firms employ a family member, and 32% have a family member who owns another firm. When two firms are owned by members of the same family, there is a 25% likelihood that they will trade, with transaction values higher than those between non-family trading partners. These patterns suggest that family ties contribute to shaping business networks, and raise questions about their impact on firm performance. Using a stacked event study design, we examine the effects of ownership transfers on firm performance, distinguishing between transfers to children or spouses, more distant family members, and unrelated individuals. Our findings show that while firms transferred within family experienced little change in sales, firms transferred to unrelated individuals increased sales by over 13%. The improvement can be attributed to the restructuring of employee and supply chain networks, as firms recruit more new employees and establish new supplier-buyer relationships.

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1 Introduction

Do family ties in businesses enhance efficiency through trust and cohesion, or do they introduce inefficiencies through nepotism? The effect is theoretically ambiguous. On one hand, family connections can help firms fill job positions more quickly and easily (Corak and Piraino, 2011; Kramarz and Skans, 2014). They can also strengthen contract enforcement, as trading partners with family relationships may trust each other enough to offer flexible payment terms (McMillan and Woodruff, 1999; Boehm and Oberfield, 2020). On the other hand, family nepotism such as hiring employees, selecting suppliers or buyers, or passing down ownership within a restricted pool of family members can lead to resource misallocation and inefficiencies (Villalonga and Amit, 2006; Pérez-González, 2006; Bennedsen et al., 2007; Bloom and Van Reenen, 2007; Bandiera et al., 2009).

Family ties exist both within firms, such as when owners employ or promote family members, and across firms, where family members who own separate businesses engage as suppliers or buyers with each other. While these ties can influence business operations in various ways, empirical evidence on their broader economic impact remains scarce. Most existing studies focus on family connections in the workforce within firms (Corak and Piraino, 2011; Kramarz and Skans, 2014), leaving the broader reach of family ties across firms unexplored. For example, we still lack a clear understanding of how common it is for firm owners to have family members who also own other firms or whether these family-related firms tend to trade with each other. Answering these questions is essential, as family networks could significantly shape broader economic outcomes, from influencing market competition to affecting resource allocation.

In this paper, we aim to fill this gap by systematically documenting the prevalence of family ties within and across businesses in the universe of an entire economy. Using unique administrative data from Taiwan, we link full-population records through family relationships, firm-level shareholder structures, firm-to-firm transactions, and employer-employee connections. Specifically, we document family connections along two dimensions. First, building on existing studies, we analyze owner-employee family relationships *within* firms, where family ties exist between owners and their employees. Second, we break new ground by documenting owner-owner family relationships *across* firms, where family members who own separate firms engage as suppliers or buyers in each other's supply chains. To our knowledge, this is the first study to use administrative data to systematically link family ties through both ownership structures and supply chain networks.

The first part of the paper presents a descriptive analysis of family ties in these two dimensions. For owner-employee family ties within firms, we find that 40% of firms employ a family member. These firms tend to be older, have lower sales, costs, and wage bills, and a higher proportion of their transactions are with businesses owned by family members. Moreover, family employees within these firms often have longer tenures and receive slightly higher wages than non-family employees, after controlling for firm fixed effects.

For owner-owner family networks across firms, we document that 33% of firm owners

have a family member who owns another firm. Among these firms with family-related owners, there is around a 55% chance that the two firms operate within the same industry. This suggests that having a family member in an industry may facilitate entry into that market, possibly due to shared knowledge or resources. Furthermore, when two firms are connected by family ties through their owners, there is a 25% likelihood that they are part of each other's supply chain, either as suppliers or buyers. We also show that the values of family-linked transactions tend to be higher than those of non-family trading partners. These patterns highlight the role of owners' family ties in shaping business networks, both within and across firms.

Building on this, the second part of the paper investigates how ownership transfers impact firm performance, focusing on the relationship between the departing and incoming owners. Ownership transfers within the family might keep the firm's structure stable, as family members often maintain established practices and trusted networks. In contrast, transfers to unrelated individuals could lead to substantial operational changes, as new owners may bring in different strategies and reshape the firm's employee and supply chain networks. To investigate this, we employ a stacked event study design (Cengiz et al., 2019) to compare firms that experience an ownership transfer with those that have never experienced such a change. Among the firms that have transferred ownership, we categorize them into three groups: (i) transfers to children or spouses, (ii) transfers to other family members, and (iii) transfers to unrelated individuals. This approach allows us to analyze how different types of ownership changes affect firm performance and understand the mechanisms behind these changes.

Our findings reveal a contrast between firms transferred to unrelated individuals and those transferred within the family. Firms transferred to unrelated individuals demonstrate immediate and substantial improvements in key performance variables. Specifically, these firms experience an increase in assets of approximately 7% in the first year, growing to about 13% four years after the transfer. Sales follow a similar trajectory, with a 13% rise observed four years after the transfer. Moreover, these firms expand their workforce significantly, hiring more new employees and increasing their total number of employees by about 18% over four years. They also broaden their supplier and client networks, establishing new relationships and engaging more with new entrants in their supply chains.

In contrast, firms transferred to children or spouses show minimal changes in performance. Their assets, sales, and workforce composition remain relatively stable, with only a slight increase in workforce size compared to the substantial growth seen in firms transferred to unrelated individuals. Firms transferred to other family members show a modest increase in assets, but otherwise display similar patterns in sales and workforce size, with changes comparable to those seen in transfers to children or spouses. The observed differences in performance between firms transferred within the family and those transferred outside may stem from variations in the reshuffling of employees and supply chain networks. One explanation is that the restructuring of employee and supply chain networks during transfers outside of the family creates better matches, which enhance productivity

and performance.

Related Literature and Contributions. This paper speaks to three strands of literature: (i) social ties and incentives in organizations, (ii) social ties and frictions in production networks, and (iii) the effect of inherited control on firm performance.

First, our research relates to the literature on social networks and incentives in organizations. Prior studies have investigated social connections among employees (Kramarz and Skans, 2014), in boardrooms (Kramarz and Thesmar, 2013), between managers and workers (Bandiera et al., 2009), and cases of kin nepotism in both the public (Riano, 2023) and private sectors (Pellegrino and Zingales, 2017; Gagliarducci and Manacorda, 2020). Unlike prior studies that often focus on specific sectors due to data limitations, our paper leverages comprehensive administrative data for an entire modern economy, allowing us to document family connections across all firms. Furthermore, our analysis goes beyond within-firm ties and explores across-firm family relationships, offering a more complete view of kinship's influence across business supply chain networks.

Second, our paper is related to the literature on social ties and friction in production networks. Previous studies have explored how factors like information asymmetries, geographic distance, and weak contract enforcement distort firms' choices of suppliers and buyers. For example, Rauch and Trindade (2002) show that ethnic networks facilitate trade by helping firms find trading partners, while Boehm and Oberfield (2020) demonstrate that weak contract enforcement can lead firms to rely on relationship-specific sourcing, potentially causing resource misallocation. To avoid contracting frictions, firms often source from relatives or maintain long-term relationships, though this may limit their ability to switch to more efficient suppliers and capitalize on new opportunities. We contribute to this literature by examining kinship ties as a unique factor in supply chains that can both reduce transaction costs and hinder firms' adaptability. By documenting the prevalence of kinship ties in production networks across an entire economy, we shed light on how family relationships shape firms' supplier and buyer choices, potentially affecting their ability to optimize operations and respond to market changes.

Third, we contribute to the literature on the impact of family control on firm performance. Previous research has yielded mixed findings on whether family ownership or management affects firm outcomes. Many studies suggest that inherited control negatively impacts performance due to issues such as nepotism and a lack of professional management (Faccio and Lang, 2002; Pérez-González, 2006; Bennedsen et al., 2007; Bertrand and Schoar, 2006; Bloom and Van Reenen, 2007), while some show that family-owned firms perform better (Anderson and Reeb, 2003). However, much of this literature relies on data from large and publicly listed firms or specific industries.

A notable exception is Bennedsen et al. (2007), which uses administrative data from Denmark to analyze the impact of family CEOs on firm performance. However, they do not investigate the mechanisms underlying these effects, leaving the sources of the negative impact unanswered. Moreover, contextual differences between Scandinavia and East

Asia may shape the role of informal ties in interfirm relationships. Family businesses are more prevalent in Asia (The Economist, 2015), and differences in social capital between the regions may shape family control and informal economic ties differently (Knack and Keefer, 1997). Our study fills these gaps by exploring the underlying mechanisms in an East Asian context. We find that firms transferred to unrelated individuals experience significant performance improvements due to restructuring in employee and supply chain networks, shedding light on the potential productivity gains from transferring ownership outside the family.

The rest of the paper proceeds as follows. Section 2 describes the data and the variable definitions. Section 3 presents the descriptive analysis of the prevalence of family ties within and across businesses. Section 4 examines the effect of ownership by type of relations on firm performance. Section 5 concludes.

2 Data & Definition

2.1 Data

Our administrative data come from five sources provided by the Taiwanese Fiscal Information Agency: (i) corporate income tax, (ii) dividend income tax, (iii) firm-to-firm value-added tax, (iv) personal income tax, and (v) population registry. All data are between 2008 and 2021.

Corporate income tax. This dataset includes publicly and privately held businesses subject to corporate income tax. Although the income threshold varies slightly by year, it is generally around 120,000 TWD (approximately 4,000 USD). The records contain detailed balance sheet items for each firm, as well as the chair of the board, who typically acts as the firm's representative.

Dividend income tax. This dataset provides information on the shareholder structures of all firms in the corporate income tax records, indicating who owns how many shares in what firms. The data are collected for the use of computing imputed tax credits under Taiwan's integrated corporate and dividend income tax system.

Valued-added tax (VAT) firm-to-firm transaction. The VAT dataset contains monthly transaction-level records of firm-to-firm sales. The income threshold for VAT liability varies by year but generally hovers around 80,000 TWD (approximately 2,670 USD). Each transaction record includes the supplier ID, buyer ID, and transaction value.

Individual income tax. This dataset provides a detailed breakdown of individual income sources on an annual basis, including salary, dividends, pension, profit, and rental income. It identifies the amount and type of income received by each individual, as well as the firms from which these payments originate.

Population registry. The population registry is a full-population dataset covering household structures and individual demographics, including gender, age, marital status, and birthplace. Crucially, it also records the IDs of each individual's parents and spouse, which is the key variable we use to identify family connections.

2.2 Sample Construction and Variable Definition

Firm sample construction. We begin with the corporate income tax data and apply the following filters to construct our firm samples: (i) firms must be limited liability companies; (ii) they must have an average monthly business revenue exceeding 1 million TWD (approximately 30,000 USD); (iii) they must be registered in the labor insurance data; (iv) owner demographic information must be available; and (v) firms must appear at least once in the VAT records between 2008 and 2021. In the final sample, we have 320,197 unique firms across all years.

Column (1) of Table 1 presents the descriptive characteristics of the firm samples. On average, firms are around 16 years old, have approximately 16 employees, with annual total wages around 8.7 million TWD. Sales and payments are roughly balanced, both averaging around 41 million TWD, indicating consistent revenue and expenditure patterns across firms. Firms typically maintain networks with around 73 suppliers and buyers, suggesting moderate interaction within supply chains. In terms of industry distribution, firms are most heavily concentrated in the retail sector, followed by manufacturing, construction, and finance.

[Table 1 here]

Definition of firm owners. Using the dividend income tax records, which provide the full shareholder structure of firms, we define a firm owner as any individual holding more than 25% of a firm's shares. For firms where no individual holds more than 25% (about 22% of the firm sample), we designate the individual with the largest shareholding as the owner. This results in 532,842 unique firm owners across the 320,197 unique firm observations.

Definition of family members of owners. To identify the family members of firm owners, we use the population registry, which records the IDs of each individual's parents and spouse. This allows us to construct a family tree for each firm owner, including the following relations (from closest to more distant): (i) children, (ii) spouse, (iii) parents, (iv) grandparents, (v) grandchildren, (vi) siblings, (vii) parents-in-law, (viii) siblings-in-law, and (ix) children-in-law. In total, we identify 5,492,075 unique family members of firm owners within the 320,197 unique firm observations.

Firm-to-firm transaction construction. Using the VAT records of our firm sample, we identify relationships between firms based on their transactions, capturing the network of suppliers and buyers. This yields 24,513,268 unique undirected dyads of firm-to-firm transactions.

3 The Pervasiveness of Family Ties Within and Across Firms

We start by presenting empirical facts regarding the pervasiveness of family ties and focus on the (i) owner-employee network within a firm and (ii) owner-owner network across firms.

3.1 Within Firms: Owner-Employee Network

To identify the relationship between firm owners and employees, we construct a year-firm-owner-employee level dataset and merge kinship information. For each year and firm, we determine if an owner and an employee within the firm are family-related. Next, we collapse the data to the year-firm level and create a binary indicator that signals whether a firm employs at least one family-related employee in a given year.

3.1.1 How Common Is Family Employment?

We begin by examining how common it is for firms to employ family members and the characteristics of these firms in Column (2) of Table 1. Out of 3,588,541 firm-year observations, 1,435,655 include a family connection between an owner and an employee, indicating that 40% of firms employ a family member. These firms tend to be older and have lower sales, costs, and wage bills than the average firm in the full sample. They also rely more on family-related business networks, as shown by a higher amount of sales and payments transacted to family trading partners. In addition, they have longer-term relationships with suppliers and clients, suggesting greater stability in the composition along their supply chains. Firms with family employees are also less likely to be publicly listed, more often operate in manufacturing, and are less common in retail and finance.

3.1.2 Do Family Employees Earn More or Stay Longer?

Given that firms employing family members tend to have smaller overall wage bills, we next examine whether family employees are paid differently or have longer tenures than non-family employees. By analyzing the wage and tenure dynamics, we can better understand the impact of family ties on employment conditions within these firms.

We estimate the following specification, distinguishing between family employees who are children or spouses of the owner and those who are other relatives:

$$Y_{ift} = \beta_0 + \beta_1 \text{ChildSpouse}_{ift} + \beta_2 \text{OthFam}_{ift} + \gamma' X_{it} + \alpha_f + \delta_t + \epsilon_{ift} \tag{1}$$

where Y_{ift} is the outcome of individual i who works in firm f in year t, which could be log wage or tenure. ChildSpouse_{ift} is 1 if individual i in firm f is the child or spouse of the owner of firm f in year t and 0 otherwise. OthFam_{ift} is 1 if individual i in firm f is other family members within the second degree of consanguinity but not a child nor spouse of the owner of firm f in year t and 0 otherwise.[1] X_{it} are control variables including i's age, age squared; α_f is firm FE and δ_t is year FE.

[1] These are, for example, parents, grandparents, grandchildren, siblings, parents-in-law, siblings-in-law, and children-in-law. See Section 2.2.

Table 2 presents the effect of being a family employee, splitting by as a child or spouse, or other relative, on wages and tenure. Column (1) shows that, on average, an employee who is a child or spouse of the owner earns around 19% less than non-family employees, while an employee who is of other family relations earns about 21.3% less. However, with firm and time fixed effects added to Column (2), the coefficients turn positive: children or spouses earn 8.3% more than non-family employees, and other relatives earn 1.6% more. The flip of the signs of the coefficients suggests that firms hiring family members tend to pay lower wages overall, but within these firms, family employees may be paid slightly more than non-family employees.

Moving to the outcome of tenure. Column (3) shows that children or spouses tend to stay longer in a firm than non-family employees, while other relatives stay for shorter periods. With time and firm fixed effects to Column (4), both groups of family employees show positive tenure effects. Children or spouses of the owner stay about 0.82 year longer and other relatives stay about 0.11 year longer than non-family employees. The change between the two columns suggests two things: first, firms that employ family members tend to have longer employee tenures overall, which explains why adding firm fixed effects reduces the tenure coefficient for children or spouses and flips the tenure coefficient of other relatives to a positive sign. Second, children and spouses still have a longer tenure compared to other relatives, even within the same firm.

[Table 2 here]

3.2 Across Firms: Owner-Owner Network

To understand the prevalence of family relationships in owner-owner networks across firms, we examine the extent to which firm owners are connected to other firms through family ties. Specifically, we define "family-related firms" as those where the owners are family members. We analyze this in two steps. First, we document how common it is for a firm owner to have a family member who owns another firm, then determine whether these family-related firms are trade with each other.

3.2.1 How Common Is It for a Firm Owner's Family Member to Own Another Firm and Trade With Each Other?

We first identify if a firm owner has a family member who owns a different firm each year. Then, we create an indicator for each firm, specifying whether or not it has a family-related firm in that year. Table 3 shows the summary statistics. Column (1) is the number of firms and Column (2) presents the fraction of firms whose owners have a family member who owns another firm. On average, 33% of firms have a family-related firm, meaning their owners are connected through a family tie.

Next, we examine whether these family-related firms tend to operate within the same industry. On one hand, family members might operate in the same industry for the use of shared knowledge, resources, or networks to facilitate business entry and growth. On the other hand, families may deliberately diversify across industries as a way to hedge against

industry-specific risks, such as market downturns or regulatory changes. Column (3) of Table 3 shows that, conditional on having a family member owning another firm, there is a 55% chance that the two firms operate within the same industry, as defined by the first two digits of the NAICS codes.

Column (4) further explores whether family-related firms trade with each other. It shows the likelihood that, conditional on having a family member owning another firm, the two firms engage in business transactions with each other, with a probability ranging from 25% to 27% across the years.

[Table 3 here]

Who are the firms that trade with family members? Are they smaller businesses that depend on family relationships for supply chain stability, or are they large, interconnected groups, such as chaebols, where families span multiple firms and use intra-family trade for strategic growth? We pool all year-firm observations across years and present their firm characteristics in Column (3) of Table 1. It shows that firms that trade with family members tend to be larger, with greater assets, more employees, higher wage bills, and higher sales and payments compared to the full sample in our data. These firms also engage with a higher number of suppliers and buyers, with transactions involving family-related firms accounting for approximately 6.6% of sales and 6.0% of costs. This pattern suggests that, at least descriptively, firms that trade with family members are more likely to be larger. However, it's important to note that engaging in family-related trade inherently conditions on having a family member who owns another firm, introducing a selection bias. To establish a causal link between family-related trade and firm performance, future analyses will need to account for this selection effect.

3.2.2 Does Family-Related Supplier-Buyer Relationship Trade More or Last Longer?

To explore the dynamics of family-related trade, we examine whether supplier-buyer relationships between family-related firms differ from those between non-family-related firms in terms of trade value and duration. We begin by constructing a year-level dataset that identifies whether a supplier's owner and a buyer's owner share a family tie. Pooling data across years yields 262,594,042 observations. For each supplier-buyer pair, we create "undirected" dyads, meaning we treat the pair as connected if a trade exists, without distinguishing which firm acts as supplier or buyer. This results in a total of 24,513,268 unique undirected dyads.

We estimate the following specification:

$$Y_{dt} = \alpha_0 + \alpha_1 \text{FamTrade}_{dt} + \gamma' X_{dt} + \mu_d + \delta_t + \epsilon_{dt} \tag{2}$$

where Y_{dt} represents either the trade value or the tenure of the dyad relationship d in year t. FamTrade_{dt} is a binary indicator that equals 1 if the relationship is family-related, i.e., if the owners of firm i and j are family-related. The vector X_{dt} includes interactions such as the product of the firm age of the two firms or the product of the firm size of the two firms.

μ_d and δ_t are dyad and time fixed effects, respectively. Standard errors are clustered on the dyad level.

The regression results are presented in Table 4. Columns (1) and (2) focus on trade value, while Columns (3) and (4) on tenure. In Column (1), family-related relationships have a much higher trade value, with a coefficient of 3.68, suggesting that trade value is over 300% higher if the transaction is family-related. However, adding dyad and time fixed effects in Column (2) reduces this to 0.813, though it remains significantly high. In other words, on average, family-related transactions are 81% higher in value than non-family-related trades.

Moving to the outcomes of tenure in Column (3), it shows that family-related trades last about 137% longer than non-family-related trades. However, after controlling for dyad and time effects in Column (4), the effect drops to near zero and becomes insignificant. This suggests that the longer duration of family-related relationships can be mostly attributed to consistent characteristics of the trading pairs or overall trends over time, rather than the family connection itself.

[Table 4 here]

4 The Effect of Ownership Transfers on Firm Performance

Our findings thus far have highlighted the prevalent role of family ties in business operations, affecting both internal employee relationships and external trading partnerships. This close integration of family ties suggests that business decisions and performance are tightly connected to the owner's family relationships. This raises a question: how does a change in ownership affect firm performance and productivity? A priori, the effect might depend on the ownership recipient. If ownership transfers to close family members, business practices are likely to stay largely the same, whereas transfers to outsiders are more likely to bring significant changes to operations and strategy.

This section examines how different types of ownership transfers affect firm performance, focusing on the relationship between the departing owner and the new owner. We aim to determine whether firms that undergo ownership transfers to children, spouses, other family members, or unrelated individuals experience different effects on firm performance.

4.1 Empirical Strategy

Selection of Transfer Events. Under our definition of firm owners, a firm could have multiple owners. To avoid complications when multiple owners change at the same time, making it hard to identify the departing and incoming owners, we limit the definition of a firm owner in this section to those registered as the chair of the board. This approach ensures that each firm has a single owner at any given time, allowing us to clearly identify the departing owner, the incoming owner, and their relationship.

We define a transfer event as a change in its owner (i.e., the chair of the board). If a firm experiences multiple transfers across years, we pick the earliest one. In addition, we apply the following restrictions: (i) the firm did not undergo another transfer within four years after the event, and (ii) the firm must have nonzero employment before the transfer. To construct our control group, we assign a placebo transfer year to firms that did not undergo any ownership transfers during our observation period. In the end, we identify 7,112 transfer events, with 4,293 involving a transfer to a child or spouse, 843 involving other family members, and 1,976 involving unrelated individuals. The control group consists of 170,412 firms that never experienced a transfer.

Firm Characteristics by Type of Transfers. Table 5 presents the summary statistics of firms, split by transfer types: never-transferred, transferred to a child or spouse, to other family members, or to unrelated individuals. Firms that transferred ownership to children or spouses tend to be associated with older departing owners, suggesting that transfers within close family are often part of generational transitions. These firms are generally smaller in terms of both assets and employees compared to firms transferred to unrelated individuals, which tend to be larger and have higher sales and payments.

In contrast, firms transferred to unrelated individuals engage with a higher number of suppliers and clients and have greater total wages and wage-per-worker averages, indicating that transfers to non-family members may correlate with larger firm size and more complex business networks. Firms transferred to other family members show characteristics in between those transferred to close family and those transferred to unrelated individuals, suggesting that the degree of family proximity may play a role in firm size and complexity post-transfer.

Stacked Event-study Design. To address issues related to negative weighting in recent dynamic difference-in-difference literature, we adopt the stacked event-study design proposed by Cengiz et al. (2019). We create event-datasets (event is the year of transfer). In each event-dataset h, the treated are the ones that underwent a transfer and the control are the ones that did not undergo a transfer throughout the studied period. We align events by relative time and stack all event datasets. The baseline regression specification is the following:

$$Y_{hik} = \sum_{k \neq -1} \beta_k \, I_{hik} + \Omega_{hik} + \mu_{hi} + \rho_{hk} + \epsilon_{hik} \tag{3}$$

where Y_{hik} is the outcome for firm i in event dataset h at relative time k. $I_{hik} = 1$ if firm i in event dataset h is transferred k years relative to the transfer year. Ω_{hik} includes firm age that varies within eventdata-firm over time. μ_{hi} is eventdata-firm fixed effect, ρ_{hk} is eventdata-time fixed effect. The control group is the firms that have never experienced an ownership transfer. We omit $k = -1$ and cluster the standard errors at the firm level.

As we are interested in the differential impact of the relation of transfer, we split the treated firms into samples based on the transfer type: (i) child or spouse, (ii) other family,

or (iii) unrelated transfers, and estimate the regression separately.

For balance-sheet outcomes that could be zero, we scale them with the average asset values before the transfer. For employee outcomes that could be zero, we scale them with the average number of employees before the transfer. For supplier-client outcomes that could be zero, we scale them with the average number of traders before the transfer.

4.2 Results

Assets, Liabilities, Sales, Purchases, Wage Bills. We start presenting the event study results estimated from Equation 3. Figure 1 displays the outcomes for assets, liabilities, sales, and costs. The estimated coefficients indicate the evolution of each outcome variable compared to firms that never underwent a transfer. Results for transfers to children or spouses are shown in black, transfers to other family members in blue, and transfers to unrelated individuals in orange. The figure shows there is no pretrend between the three types of treated group and the control group, lending support to the parallel trend assumption.

Panel A illustrates the changes in assets after the transfers. Firms transferred to unrelated individuals experienced an immediate increase in assets of approximately 7%, which continued to grow to about 13% four years after the transfer. Firms transferred to other family members also saw an increase in assets, but the growth was smaller compared to transfers to unrelated individuals. By the end of the fourth year, the change in assets is around 6%. In contrast, firms transferred to children or spouses showed little to no change in assets following the transition.

To explore where the source of change in assets is from, Panel B plots the trend of liabilities. Firms transferred to unrelated individuals saw a rise in liabilities, increasing by around 10% a year after and reaching 18% by year four. For firms transferred to other family members, liabilities showed a modest, statistically insignificant increase. In contrast, firms transferred to children or spouses saw no liability increase initially, and by year three, liabilities had slightly decreased, averaging a 0.3% decrease by year four. This trend may suggest that family-transferred firms, particularly those given to children or spouses, are less likely to finance investments, possibly reflecting a more cautious approach to risk-taking after the transition.

In Panel C, we examine sales. Firms transferred to unrelated individuals increased their sales by around 8% a year after the transfer, with growth reaching around 13% four years after. Transfers to other family members also displayed an upward trend in sales, although the estimates are statistically insignificant. In contrast, firms transferred to children or spouses showed a slight, statistically insignificant downward trend in sales following the transition.

Panel D shows the trend in purchases. Firms transferred to unrelated individuals increased their purchases for production, averaging 12% more in the first year after the transition, with the increase stabilizing over the following four years. For firms transferred to other family members, the effect was statistically insignificant but showed slight growth starting in the second year after the transition. Firms transferred to children or spouses did

not exhibit significant changes in their payment bills.

Panel E illustrates that firms transferred to unrelated individuals increased their total wage bills by approximately 3% in the first year after the transfer, with growth continuing to more than 4% four years later. In contrast, firms transferred to other family members or to children or spouses did not exhibit any significant changes in their total wage bills.

Number of Employees and Entrant Employees. Is the increase in total wage bills related to the change in the number of employees? Panel A of Figure 2 examines the evolution of employee numbers. To account for the issue of zeros, we scale the number of employees by the mean of the number of employees before the transfer. It shows that firms transferred to unrelated individuals began increasing their employee count after the transfer, with an 8% rise in the first year and growth reaching about 19% four years later. Firms transferred to other family members or to children or spouses also saw increases in employee numbers, but the growth was smaller and occurred more gradually. Four years after the transfer, the increases in employee numbers were 10% for firms transferred to other family members and 4% for those transferred to children or spouses.

Panel C shows the evolution of new employee hires. The results indicate that firms undergoing an unrelated transfer hired approximately 20% more new employees after the transition, with continued hiring in subsequent years. In contrast, firms transferred to other family members or to children or spouses mainly hired new employees during the year of the transition and the following year, but the magnitude was much lower compared to firms transferred to unrelated individuals.

Number of Suppliers, Buyers, and Entrants. Are the increases in sales and costs linked to changes in suppliers and clients? Figure 3 uses the same empirical strategy to explore the number of suppliers, clients, and new entrants in the supply chain. Again, we detect no pretrends between the firms that are transferred versus never transferred.

Panel A presents the evolution in the number of suppliers. Similarly, to account for the issues of zeros, we scale the number of suppliers by the mean value of the number of suppliers before the transfer. Starting from the year of transition, all three types of transferred firms experienced an upward trend in supplier numbers. However, the differences in magnitude became notable one year after the transfer. Firms transferred to unrelated individuals saw a 7% increase in the number of suppliers compared to the base period, with continued growth reaching around 10% four years later. In contrast, firms transferred to family members increased their supplier numbers by approximately 3% one year after the transfer, with a smaller, slower growth trajectory that leveled off at under 5% after four years.

To investigate whether the increase in supplier numbers stems from new entrants to the supply chain, we analyze the number of entrant suppliers in Panel B. The definition of an entrant is it is the first time the supplier appears in the supply chain as a supplier. The results show that firms transferred to unrelated individuals saw an around 6% increase in the number of entrant suppliers in the first year after the transition, and the number

oscillated around 5% by the end of the fourth year, while firms transferred within the family resulted in an increase of only around 2%.

Moving to the buyers, we examine whether a similar pattern exists. Panel C shows the trend in the number of buyers over time. Firms transferred to unrelated individuals experienced a 3% increase in buyers one year after the transfer, with the trend continuing upward to reach 4% four years later. In contrast, the changes in buyer numbers for firms transferred to other family members are statistically insignificant, while those transferred to children or spouses show a slight, statistically insignificant upward trend.

Panel D examines whether the increase in buyer numbers following transfers to unrelated individuals can be attributed to new entrant buyers. Firms transferred to unrelated individuals saw an average increase of around 2% in new buyers one year after the transition, with this trend remaining stable over subsequent years. The trend for firms transferred within the family, however, is statistically insignificant.

These findings suggest that firms transferred to unrelated individuals not only increase their supplier and client bases but also are matched with more entrants within the supply chain, particularly on the supplier side. In contrast, firms transferred within the family exhibit more modest and generally insignificant changes in their supply and client networks.

Discussion. The results point to key differences in how firms react to ownership transfers based on the type of recipient. For firms transferred to unrelated individuals, we observe significant increases in assets, sales, costs, liabilities, and the expansion of employees and supplier and client networks. These changes suggest that unrelated recipients may adopt a growth-oriented approach, possibly by investing in new projects, increasing workforce, and forming connections with new suppliers and buyers. The hiring of new employees and engagement with new suppliers and buyers may indicate a drive for business expansion or diversification under new ownership.

In contrast, firms transferred within the family, especially to children or spouses, show more conservative patterns. The limited or statistically insignificant changes in assets, liabilities, and supplier or buyer networks suggest that these family-transferred firms may be less inclined to change the existing networks. This trend could reflect a preference among family members to maintain stability rather than pursue growth, potentially due to a desire to preserve legacy or minimize financial risks. The slight decrease in liabilities observed for firms transferred to children or spouses might further support the notion of conservative financial management.

5 Conclusion

This paper paints a comprehensive picture of family ties along two dimensions of firms: owner-employee family links within firms and owner-owner family links across firms. By documenting these relationships, we provide new insights into the widespread nature of family connections in firm operations and performance. Using a stacked event study to an-

alyze the effect of ownership transfers on firm performance, we find that firms transferred to unrelated individuals experience significant improvements in performance compared to those transferred within the family. The improved performance could be driven by the restructuring of employee networks and supply chain relationships.

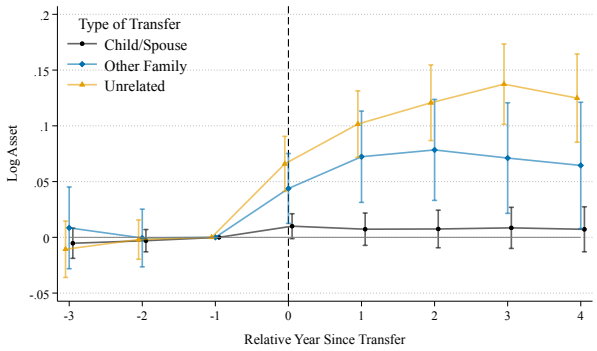
Our study highlights the broader economic effects of family networks within firms and in business transactions across firms. While family control can provide stability and preserve established relationships, our findings suggest that firms may perform better when ownership is transferred to unrelated individuals. These transfers may lead to changes that create better matches with employees, suppliers, and clients, which results in higher productivity.

These findings have important policy implications, for example, for estate and inheritance tax relief policies that encourage family business succession by offering lower tax rates for keeping businesses within families. While such policies aim to support family continuity and stability, they may unintentionally hold back firm growth and productivity.

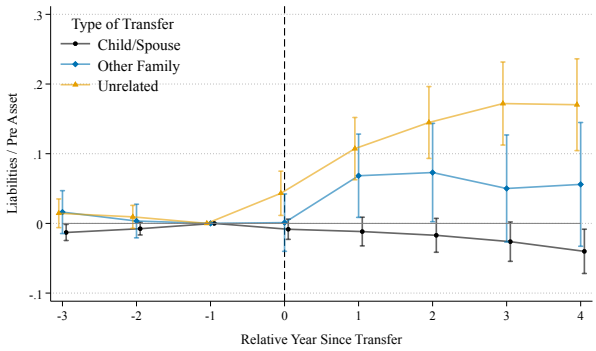
Figures

Figure 1: The Effect of Different Types of Transfers on Different Outcomes

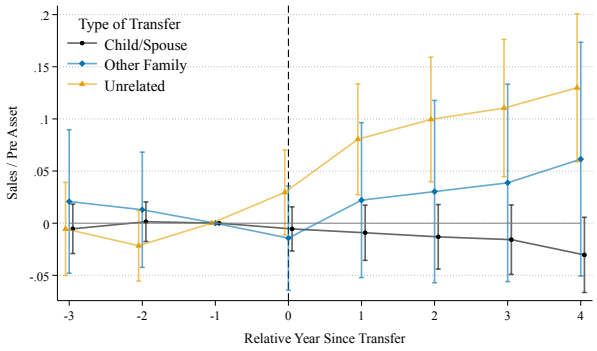
(A) Log Asset



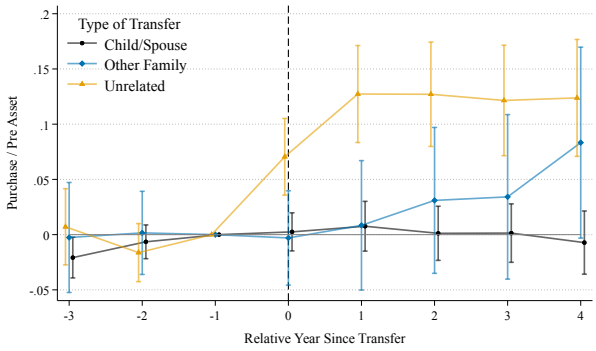
(B) Liabilities / Pre Asset



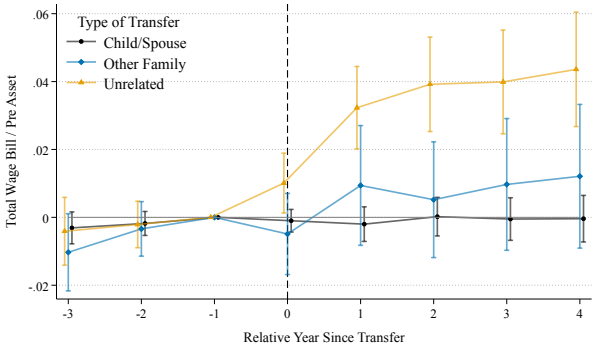
(C) Sales / Pre Asset



(D) Costs / Pre Asset



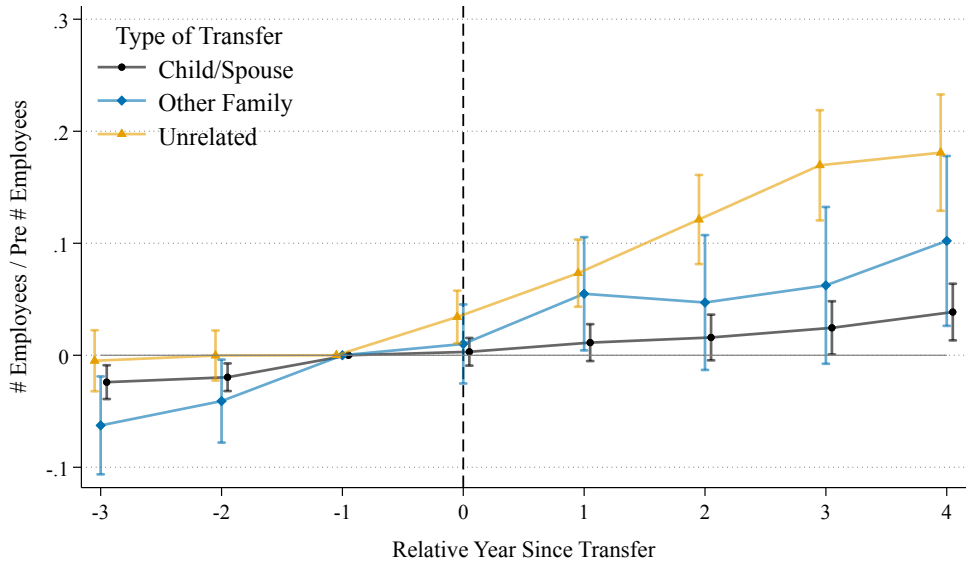
(E) Total Wage Bills / Pre Asset



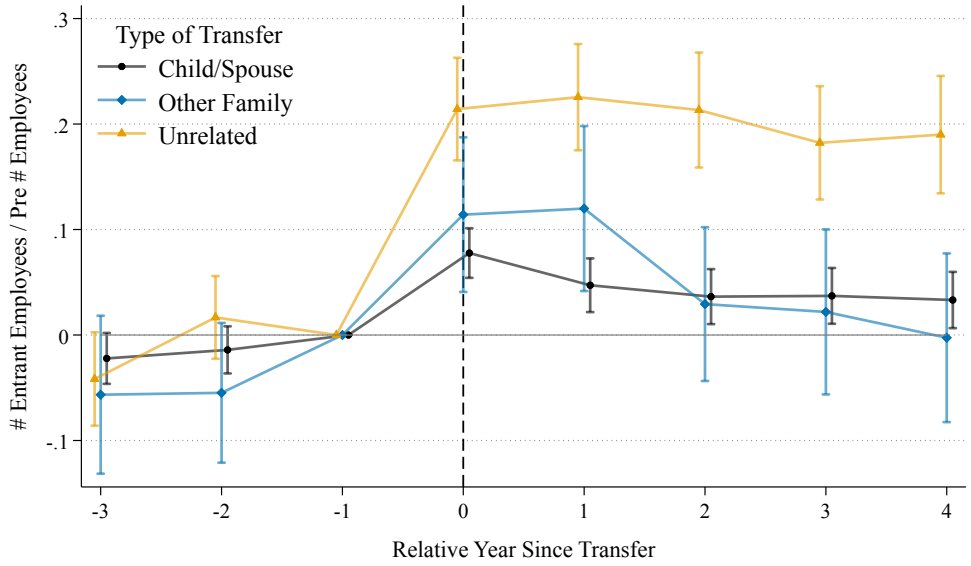
Notes: This figure plots the estimated β_k coefficients, along with their 95% confidence intervals, from Equation (3). The black line represents the estimated coefficients for the regression where the treated group comprises firms transferred to children or spouses. The blue line corresponds to transfers to other family members. The orange line represents transfers to unrelated individuals. These coefficients capture the average differences in outcomes between each type of transfer and never-transferred cases. In Panel A, the dependent variable is the log of asset values. In Panel B, the dependent variable is liabilities scaled by the mean asset value before the transfer. In Panel C, the dependent variable is sales scaled by the mean asset value before the transfer. In Panel D, the dependent variable is purchases scaled by the mean asset value before the transfer. In Panel E, the dependent variable is the total wage bill scaled by the mean asset value before the transfer. Click here to return to the text.

Figure 2: The Effect of Different Types of Transfers on Different Outcomes

(A) # Employees / Pre #Employees

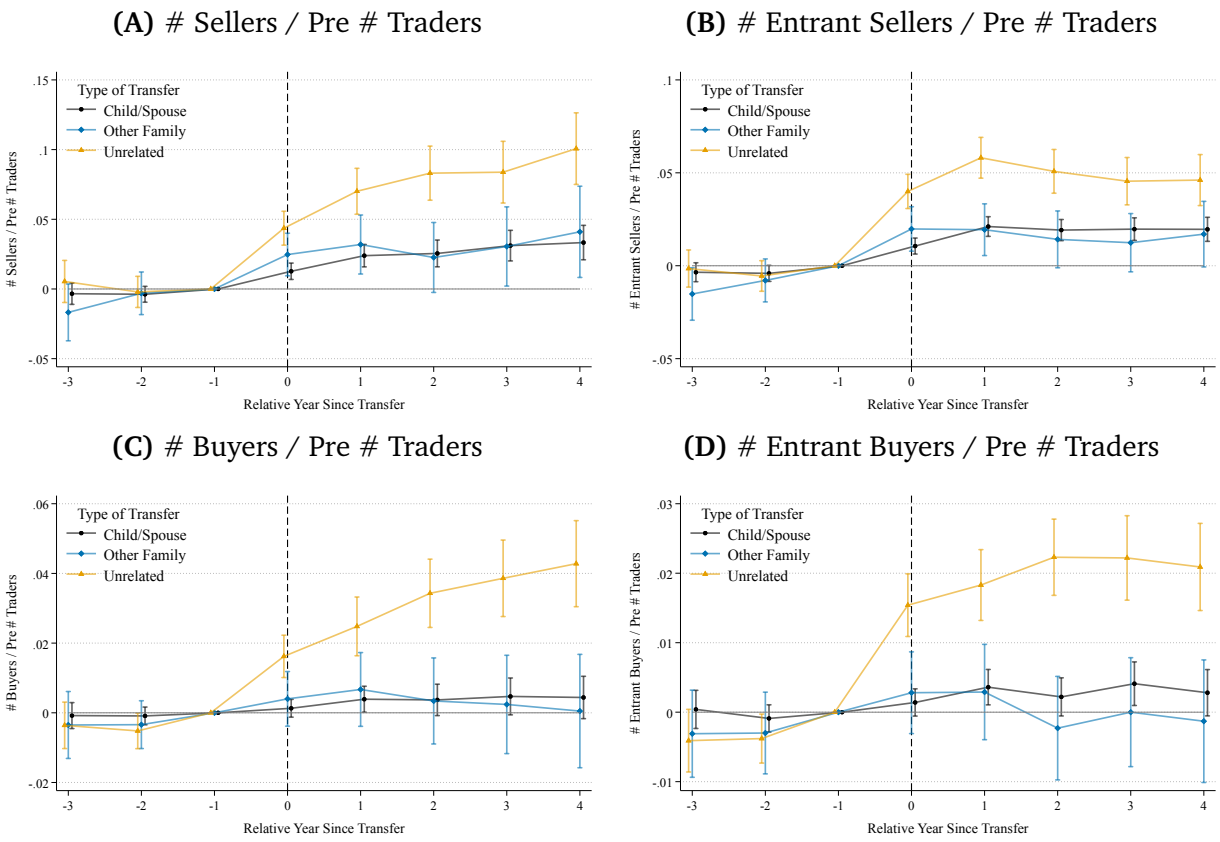


(B) # Entrant Employees / Pre # Employees



Notes: This figure plots the estimated β_k coefficients, along with their 95% confidence intervals, from Equation (3). The black line represents the estimated coefficients for the regression where the treated group comprises firms transferred to children or spouses. The blue line corresponds to transfers to other family members. The orange line represents transfers to unrelated individuals. These coefficients capture the average differences in outcomes between each type of transfer and never-transferred cases. In Panel A, the dependent variable is the number of employees, scaled by the mean number of employees before the transfer. In Panel B, the dependent variable is the number of entrant employees, defined as those who first appear in the wage bill, scaled by the mean number of employees before the transfer. Click here to return to the text.

Figure 3: The Effect of Different Types of Transfers on Different Outcomes



Notes: This figure plots the estimated β_k coefficients, along with their 95% confidence intervals, from Equation (3). The black line represents the estimated coefficients for the regression where the treated group comprises firms transferred to children or spouses. The blue line corresponds to transfers to other family members. The orange line represents transfers to unrelated individuals. These coefficients capture the average differences in outcomes between each type of transfer and never-transferred cases. In Panel A, the dependent variable is the number of sellers, scaled by the mean number of traders (i.e., sellers + buyers) before the transfer. In Panel B, the dependent variable is the number of entrant sellers, defined as those who first appear in the supply chain as a supplier, scaled by the mean number of traders before the transfer. In Panel C, the dependent variable is the number of buyers, scaled by the mean number of traders before the transfer. In Panel D, the dependent variable is the number of entrant buyers, defined as those who first appear in the supply chain as a buyer, scaled by the mean number of traders before the transfer. Click here to return to the text.

Tables

Table 1: Firm Characteristics by Whether Employ or Trade With a Family Member

	(1)	(2)	(3)
	All	Have Family	
		Employees	Suppliers/Buyers
Log Assets	16.450	16.693	17.105
	(0.001)	(0.001)	(0.003)
Firm age	15.899	17.971	16.884
	(0.006)	(0.008)	(0.020)
# Employees	16.071	16.601	22.571
	(0.073)	(0.063)	(0.215)
Total wage	8771286	8313458	12200000
	(62716.72)	(56429.28)	(261918)
Sales	41100000	38100000	60200000
	(574035.8)	(466743.8)	(983536.2)
Payments	41100000	38800000	66100000
	(428528.6)	(478004.3)	(1455795)
# Suppliers	73.176	84.176	97.063
	(0.052)	(0.065)	(0.198)
# Buyers	73.176	76.664	116.856
	(0.488)	(0.520)	(1.806)
Sales to Fam Buyers	394457.7	459592.1	3987547
	(7269.04)	(10528.58)	(73207.23)
Payments to Fam Suppliers	394457.7	495784.2	3987547
	(7130.86)	(11320.25)	(71804.97)
Seller tenure	3.3497	3.5217	3.3833
	(0.0009)	(0.0012)	(0.0029)
Buyer tenure	3.5753	3.8256	3.6517
	(0.0014)	(0.0018)	(0.0041)
% Publicly-listed	5.0	3.8	5.9
	(0.004)	(0.004)	(0.010)
% Agricultural	0.54	0.43	0.46
	(0.004)	(0.005)	(0.010)
% Manufacturing	22.48	28.05	26.27
	(0.020)	(0.030)	(0.070)
% Construct	12.97	12.09	10.69
	(0.020)	(0.020)	(0.050)
% Retail	46.90	46.50	49.13
	(0.030)	(0.040)	(0.080)
% Restaurants	6.09	4.62	5.23
	(0.010)	(0.020)	(0.040)
% Financial & Other	11.02	8.32	8.22
	(0.010)	(0.010)	(0.030)
N	3,588,541	1,435,655	309,673

Notes: The table presents the characteristics of firms in our sample. Column (1) represents the full sample of firms we constructed. Column (2) restricts the sample to firms that employ at least one family member. Column (3) includes firms that have a supplier or buyer relationship with another firm where the owners are family-related. Click here to return to the text.

Table 2: Wage and Tenure Outcomes of Family Employees

	(1) Log wage	(2) Log wage	(3) Tenure	(4) Tenure
ChildSpouse	-0.189***	0.083***	1.032***	0.815***
	(0.000)	(0.000)	(0.003)	(0.007)
OthFam	-0.213***	0.016***	-0.176***	0.107***
	(0.000)	(0.002)	(0.004)	(0.008)
Control	V	V	V	V
Time FE		V		V
Firm FE		V		V
N	45,823,562	45,823,562	42,966,650	42,966,650

Notes: This table presents the results from Equation (1). The first two columns use log wage as the dependent variable and the last two use tenure (unit is year). Click here to return to the text.

Table 3: Likelihood of Having Family Members Owning Another Firms

Year	(1) #Firm	(2) % Has family who owns another firm	(3) Among (2) within same industry	(4) Among (2) who trades with each other
2008	210474	33.6%	54.8%	25.7%
2009	214514	33.7%	54.8%	25.3%
2010	220307	33.7%	54.8%	25.5%
2011	226202	33.8%	54.6%	25.6%
2012	230995	33.8%	54.9%	25.6%
2013	235692	33.8%	54.8%	25.8%
2014	240276	33.6%	54.7%	25.8%
2015	244582	33.5%	54.8%	26.0%
2016	249298	33.5%	54.6%	25.8%
2017	252438	33.3%	54.6%	25.8%
2018	252953	33.0%	54.5%	26.1%
2019	255292	32.9%	54.4%	26.2%
2020	254544	32.5%	54.4%	26.4%
2021	252815	32.1%	54.2%	26.9%

Notes: This table presents summary statistics on the prevalence of owner-owner family ties across businesses over time. Column (1) shows the number of firms. Column (2) displays the fraction of firms whose owners have a family member that owns another firm. Column (3) indicates the likelihood, conditional on having a family-related firm, that the two firms operate in the same industry. Column (4) presents the likelihood, conditional on having a family-related firm, that the two firms first trade with each other (as a supplier or buyer). Click here to return to the text.

Table 4: The Relationship between Trade Values and Tenures of Family-Related Transactions

	(1) Log Trade Value	(2) Log Trade Value	(3) Tenure	(4) Tenure
famTrade	3.678*** (0.007)	0.813*** (0.011)	1.366*** (0.010)	0.001 (0.007)
Control	V	V	V	V
Dyad FE		V		V
Time FE		V		V
N	67,675,249	67,675,249	67,675,249	67,675,249

Notes: This table presents the results of Equation (2). The dependent variable in Column (1) and (2) is the log of the trading amount and that of Column (3) and (4) is the tenure of the relationship (unit is year). Click here to return to the text.

Table 5: Firm Characteristics by Type of Ownership Transfer

	(1) Never-treated	(2) Child/Spouse	(3) Other Family	(4) Unrelated
Departing owner age	53.43	59.42	54.06	55.22
Incoming owner age	53.24	48.17	51.24	49.24
Log Assets	16.67	16.86	16.96	17.48
	(0.00)	(0.01)	(0.02)	(0.02)
Log Revenues	16.93	16.98	17.12	17.53
	(0.00)	(0.01)	(0.02)	(0.02)
Firm age	18.55	22.69	18.31	18.00
	(0.00)	(0.06)	(0.15)	(0.09)
# Employees	15.10	15.96	19.27	28.96
	(0.01)	(0.16)	(0.44)	(0.38)
Total wage	6684082.00	6802466.00	8439771.00	15300000.00
	(5061.85)	(79740.91)	(220093.10)	(212810.80)
Wage per worker	278113.00	274342.90	268087.90	320807.10
	(51.93)	(766.44)	(1795.50)	(1434.11)
# Suppliers	81.25	82.91	88.41	107.15
	(0.03)	(0.42)	(1.07)	(0.84)
# Clients	55.66	61.86	58.46	67.12
	(0.04)	(0.72)	(1.61)	(1.19)
Tenure seller	3.64	3.76	3.51	3.37
	(0.00)	(0.01)	(0.02)	(0.01)
Tenure client	3.90	4.18	3.83	3.59
	(0.00)	(0.01)	(0.03)	(0.02)
Sales	29800000.00	32200000.00	38500000.00	57500000.00
	(22735.03)	(391250.10)	(1034383.00)	(893989.80)
Payment	28300000.00	30500000.00	36500000.00	56500000.00
	(24040.19)	(418137.30)	(1063958.00)	(927794.30)
Self Sales	115049.10	118566.30	133305.20	166439.00
	(255.08)	(4249.41)	(10062.05)	(7303.18)
Family Sales	110567.90	105639.80	174547.30	153106.70
	(239.55)	(3739.27)	(10772.35)	(6956.54)
Self Payment	111834.90	110913.80	135985.20	157890.30
	(246.70)	(4014.14)	(9859.76)	(6936.96)
Family Payment	111779.30	113234.80	170348.80	151478.20
	(240.34)	(3941.77)	(10764.72)	(6788.97)
% Publicly-listed	0.67	0.46	1.05	5.04
	(0.00)	(0.00)	(0.00)	(0.00)
% Agricultural	0.38	0.42	0.62	0.74
	(0.00)	(0.00)	(0.00)	(0.00)
% Manufacturing	27.06	36.96	31.52	27.18
	(0.00)	(0.00)	(0.01)	(0.00)
% Construction	11.66	8.88	10.99	10.86
	(0.00)	(0.00)	(0.00)	(0.00)
% Retail	48.25	43.88	42.72	40.99
	(0.00)	(0.00)	(0.01)	(0.00)
% Restaurants	5.56	4.63	5.78	8.74
	(0.00)	(0.00)	(0.00)	(0.00)
% Financial Real Estate	2.14	2.16	4.08	4.81
	(0.00)	(0.00)	(0.00)	(0.00)
% Other	4.95	3.07	4.29	6.69
	(0.00)	(0.00)	(0.00)	(0.00)
Unique N	170412	4293	843	1976
N	8829338	34344	6744	15808

Notes: This table presents the firm characteristics of those that have never experienced a transfer (Column (1)), were transferred to children or spouses (Column (2)), were transferred to other family members (Column (3)), and were transferred to unrelated individuals (Column (4)). Click here to return to the text.

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Footloose Enough? Multinationals, Domestic Firms, and Job Creation in Developing Economies as Wages Rise

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January 2025

Abstract

Job creation is the foremost policy goal in most developing countries. The ambition is for this to take place in "upgrading" domestic firms, but policies designed to encourage such upgrading — such as formalisation incentives, minimum wages, export promotion — are typically implemented alongside efforts to attract multinational enterprises (MNEs). The process of job creation and wage growth is challenging to initiate without the advanced technology, economies of scale, and positive spillovers that multinationals bring to domestic firms. Rising wages, however, tend to disproportionately benefit high-paying firms, such as multinationals, relative to lower-paying domestic firms. Can multinationals, over time, come to inhibit the domestic development process they may initially stimulate? And if they do, do they "overstay their welcome" or move on to the next low-wage host country to render any such static/dynamic tension moot? In this project, we study these questions by linking representative 2006-2021 data on foreign and domestic-owned establishments in Vietnam, firm-wide data on the Japanese-owned establishments, a survey of the Japanese multinationals' wage-setting practices and location decisions, and a 2011 reform that dramatically increased Vietnam's minimum wage. In early-stage findings we find that the minimum wage hike decreased employment at Vietnamese-owned establishments, while the employment impact at multinationals' establishments—whose wages were typically higher than the new minimum wage—is positive; initially small; and growing over time. The survey helps explain this finding and confirms that multinationals' headquarters typically absorb increased labour costs in their foreign establishments.

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This study is conducted as a part of the Project "Innovation, Globalisation and Employment" undertaken at the Research Institute of Economy, Trade and Industry (RIETI).

1 Introduction

Foreign direct investment (FDI) is a key driver of economic development and structural transformation. A growing number of multinational enterprises (MNEs) operate in developing countries, creating employment opportunities and facilitating technology and knowledge transfer. Although the incentives for FDI vary across firms and depend on the economic conditions of both the home and destination countries, outsourcing represents a primary reason for investment. This is particularly relevant for manufacturing firms with labour-intensive production process. Vertical FDI often takes advantage of wage differentials between countries, allowing MNEs to outsource production processes at lower labor costs. Offshoring to low-wage countries frequently substitutes for domestic manufacturing employment in high-wage countries (Harrison and McMillan, 2011; Muendler and Becker, 2010).

While wage gaps attract MNEs, low- and middle-income countries have experienced faster wage growth than high-income countries. Between 2016 and 2019, real wages grew by 3.5–4.5% annually in emerging G20 economies, compared to slower growth of 0.4-0.9% in advanced G20 countries (ILO, 2021). Despite these trends, little is known about how MNEs adapt to rising wages of host countries over the long run. Muendler and Becker (2010) argues that MNEs' employment decisions primarily takes place at the extensive margin (upon entry/exit) in response to the wage differentials, whereas adjustments at the intensive margin (within existing affiliates) may be limited in low-wage locations. However, how the dynamics of labour substitution between headquarters and foreign subsidiaries evolve over time, particularly in the context of shrinking wage differentials, remains underexplored.

This paper addresses this knowledge gap by analysing Vietnam's 2011 minimum wage reform with a particular focus on its spillover effects on Japanese MNEs' headquarters. Since the bilateral trade liberalisation with the United States in 2001, Vietnam has

attracted a growing number of foreign enterprises. MNEs, particularly from East Asian countries, have significantly expanded their investments in Vietnam, contributing to the growth of formal employment in the manufacturing sector (McCaig and Pavcnik, 2018, 2023). Japan ranks third in terms of the number of subsidiaries operating in Vietnam. While Vietnam has experienced rapid wage growth in recent decades, Japan's wages have remained stagnant. This disparity creates a unique setting to examine MNE responses when only host-country wages increase.

In 2011, Vietnam introduced a minimum wage reform, leading to a significant increase in minimum wages for foreign and domestic firms. Prior to the reform, foreign firms were required to comply with higher minimum wages than domestic ones, which was unified for both groups. The reform also reclassified a large fraction of districts into higher wage categories. As a result, both domestic and foreign firms experienced substantial wage increases, with domestic firms facing a greater impact. On average, minimum wages increased by 49.7% for foreign firms and 102.7% for domestic firms in one year. These increases were significantly larger than prior and posterior annual changes, which typically did not exceed 20%.

We identify the impact of this quasi-experimental wage shock through three approaches. First, we leverage rich firm-level administrative data in Vietnam to estimate heterogeneous effects on foreign and domestic firms over a ten-year period following the reform. We exploit firm-level exposure to minimum wage shock, defined by pre-reform average wages, and use an event study method with two-way fixed effects. The results indicate that both foreign and domestic firms significantly increased their labour costs following after the reform. However, there is a significant variation in their employment responses; domestic firms mitigated the impact by significantly reducing employment, whereas MNEs showed minimal employment adjustments. These effects persisted over time. These findings suggest that foreign firms absorbed labour cost increases, while

domestic firms adjusted the labour cost by reducing employment.

Second, by constructing a comprehensive panel dataset linking labour market outcomes of Vietnamese subsidiaries to their Japanese headquarters, we investigate the long-term impacts on home-country labour substitution. A similar event-study approach shows that an increase in subsidiaries' labour costs in Vietnam did not lead to changes in employment at Japanese headquarters. We find no evidence of spillover effects, suggesting that MNEs do not adjust their labour substitution dynamics even in response to significant labour cost increases.

Finally, we conduct an original survey of Japanese MNEs to understand the mechanisms through which they respond to the wage increases in the destination country. The survey reveals that MNEs increase the wage levels of subsidiaires in response to the wage increases of local domestic firms. Furthermore, while reduction of local employees is a common strategy, a larger share of MNEs investing in Vietnam reported taking no action in response to rising wage levels. More than 40% of surveyed firms reported long-run profit losses in their subsidiaries due to increased labour costs. Our findings highlight the limited extent of MNEs' employment adjustments at the intensive margin, consistent with Muendler and Becker (2010) and Harrison and Scorse (2010).

This study contributes to two strands of literature. First, it extends the FDI literature by examining the labour market implications of MNE operations. Prior research highlights the role of MNEs in increasing local employment and wages through both direct and indirect effects. The direct effect arises from providing job opportunities (Mendola et al., 2024), while the indirect effect stems from knowledge and technology transfers, etc. (Jiang et al., 2018; Keller, 2021; Setzler and Tintelnot, 2021; Abebe et al., 2022)[1]. MNEs generally offer higher wages than domestic firms and attract workers from

[1]MNEs' entry does not necessarily result in positive impact in the local labour market. Alfaro-Ureña et al. (2022) argues that the responsible sourcing standard of MNEs resulted in positive gains for low-wage workers exposed in the supply chain, the majority of low-wage workers experienced negative effect due to indirect effect and price index.

local enterprises (Hijzen et al., 2013; Javorcik, 2015). Several factors contribute to this wage premium, including higher hiring and training costs (Alfaro-Ureña et al., 2022) and superior management practices (Bloom et al., 2012). Moreover, shocks in MNEs' home countries can significantly influence wages in host countries (Hjort et al., 2022). While most studies in this area have analysed the impact of MNE entry and operations on local labour markets, this paper stands out by exploiting a local policy shock to investigate how MNEs respond to external wage increases in the host countries [2]. Our findings highlight that MNEs do not significantly adjust to wage increases through employment, instead passively absorb the wage changes.

Second, the paper contributes to the literature on minimum wage policies and their labour market effects (see Dube and Lindner, 2024 for review). Previous research has found minimal employment effects in various contexts (see, for example, Card and Krueger, 1994; Aaronson and French, 2007; Harasztosi and Lindner, 2019; Rao and Risch, 2024), while several studies show the negative effects on manufacturing employment (Cengiz et al., 2019; Gopalan et al., 2021; Harasztosi and Lindner, 2019). Our results suggest that domestic manufacturing enterprises experience significant employment reduction, which is consistent with the literature. Additionally, we extend the discussion by exploring the heterogeneity between MNEs and domestic firms, contributing to debates on wage inequality(Autor et al., 2016; Bossler and Schank, 2023). Vietnam's 2011 reform offers a unique case due to its magnitude [3], coverage and the differing impacts on domestic and foreign firms. Few literature has explored this reform, except for Nguyen (2023) that examines the impact of minimum wage reform in Vietnam on firms' product switching. Moreover, there is limited evidence that exploits firm-level exposure to the

[2]The effect on the employment of home countries is analysed by Alviarez et al. (2022) and Lin et al. (2024), while the shocks they exploit rather affect FDIs at the extensive margin (entry/exit), while the minimum wage shock that we study concerns firms' adjustment at the intensive margin.

[3]Most literature studies the impact of rather small-scale minimum wage changes, while the shocks we see accounts for 50-100% increase in minimum wages. There is limited number evidence on such large shock, namely Harasztosi and Lindner (2019) and Giupponi et al. (2024).

minimum wage (Machin et al., 2003; Draca et al., 2011; Rao and Risch, 2024) and cases from low- or middle-income countries (Broecke et al., 2017; Higashigata, 2021; Neumark and Munguía Corella, 2021). While further research is necessary to explore mechanisms such as labour reallocation[4], our findings expands the discussion of heterogeneous impact of minimum wage changes.

The remainder of this paper is organised as follows: section 2 discusses the 2011 minimum wage reform, section 3 describes the datasets and construction of the merged panel, section 4 presents the results for firms in Vietnam, and section 5 examines the outcomes for Japanese headquarters. The paper concludes with a discussion in section 6.

2 Context

2.1 Minimum wage policies in Vietnam

Vietnam first introduced minimum wages[5] for foreign enterprises in 1992 and general minimum wages for employees in public bodies in 1995. Domestic private firms used the state enterprises' minimum wage as a benchmark, although their wage settings were significantly lower than those of foreign firms (Nguyen, 2023). In 2006, the country introduced the current regional minimum wage system, where wage levels are determined at the district level[6] This system comprises four regional classifications for minimum wage standards. Region I, with the highest minimum wage, includes major cities such as Hanoi and Ho Chi Minh City, while Region IV covers rural areas[7]. Domestic private firms were required to adhere to regional minimum wages, but their standards remained lower than those for foreign firms at the beginning. The government updates minimum wage levels

[4]Research suggests that minimum wages reallocate workers to higher-productivity firms, mitigating potential employment losses (Dustmann et al., 2021)

[5]Although the Labour Code specifies that minimum wages are set for monthly, daily, and hourly standards, stakeholders most exclusively focus on monthly wages (Schmillen and Packard, 2016)

[6]Vietnam has 708 districts as of 2020.

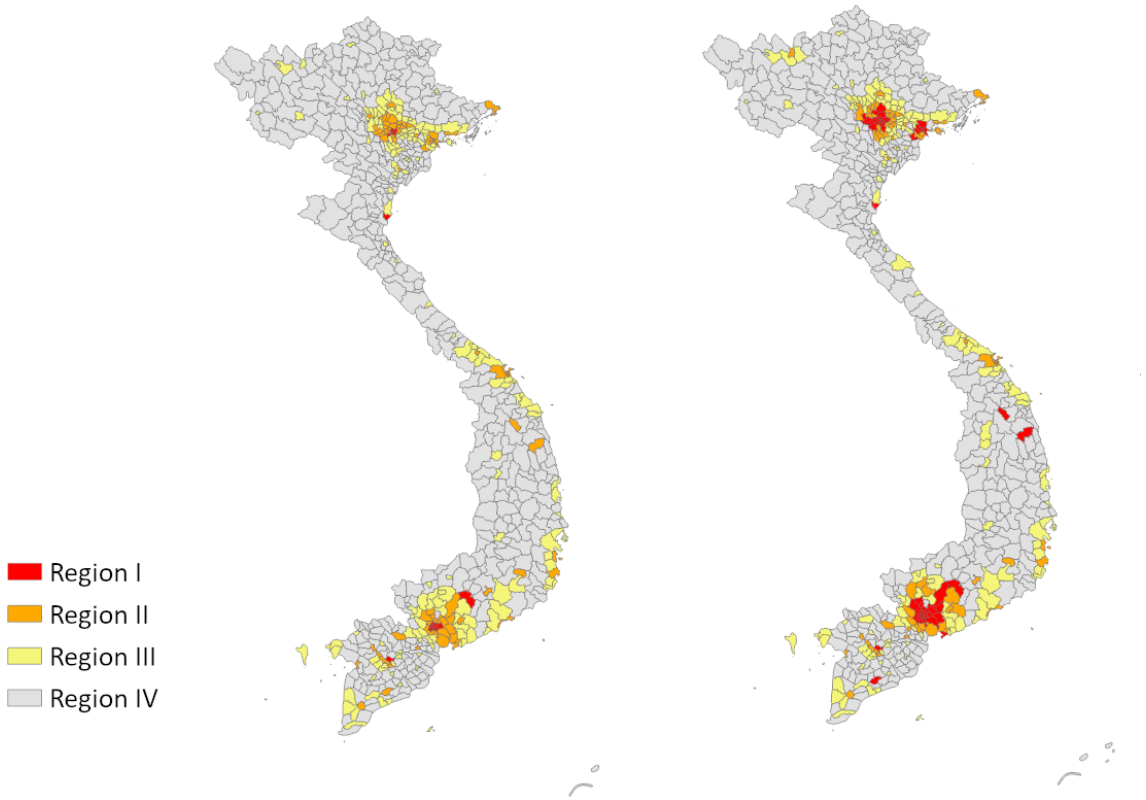
[7]In 2020, 78 districts were classified as Region I, 87 as Region II, 178 as Region III, and 365 as Region IV.

almost annually through issuing decrees, which also define the classification of districts into the four regional categories in the appendix. No district has been downgraded to a lower wage category, and districts that are upgraded experience larger increases in minimum wages compared to those remaining in the same category.

Vietnam introduced a minimum wage reform in 2011, which resulted in a significant increase in minimum wage standards. The reform had two key components: First, prior to the reform, foreign and domestic firms operated under different minimum wage standards, which were unified in October 2011. Until 2010, foreign firms were subject to higher minimum wages than domestic firms within the same region. Domestic private firms' minimum wages were set at approximately 75% of those for foreign firms, although domestic firms saw faster growth in minimum wages during the pre-reform period. From October 2011, minimum wages were unified at 2 million VND in Region I, 1.78 million VND in Region II, 1.55 million VND in Region III, and 1.44 million VND in Region IV. Table A1 shows the transition of minimum wages across regions.

Second, 2011 saw the largest number of districts upgraded to higher wage categories. Figure 1 highlights the expansion of districts with higher minimum wage classifications. Until 2010, 29 districts were classified as Region I, 54 as Region II, 111 as Region III, and 514 as Region IV. Following the reform, these numbers increased to 68, 70, 155, and 415, respectively. The upgrades were concentrated in the suburbs of Hanoi and Ho Chi Minh City. In just one year, 193 out of 708 districts (27.2%) experienced category upgrades. This expansion was extraordinary, as in other years, upgrades typically affected no more than 30 districts annually.

As a result of this reform, both domestic private firms and foreign firms experienced substantial increases in minimum wages, while the impact was greater for domestic firms. Figure 2 illustrates the distribution of minimum wage changes by district. Foreign firms saw an average increase of 49.7% in minimum wages, while domestic firms experienced



(a) December 2010 (b) December 2011

Figure 1: Regional categories of minimum wage

Note: (a) uses the minimum wage standard from decree in January 2010, which was valid until January 2011. (b) is the category defined in the decree of October 2011, which was in force until January 2013.

an average rise of 102.7%. This was the largest single-year jump in minimum wages, as previous and subsequent annual increases rarely exceeded 20%. The extremely large magnitude in minimum wage increase makes the case unique setting to study the firm-level responses to the shock.

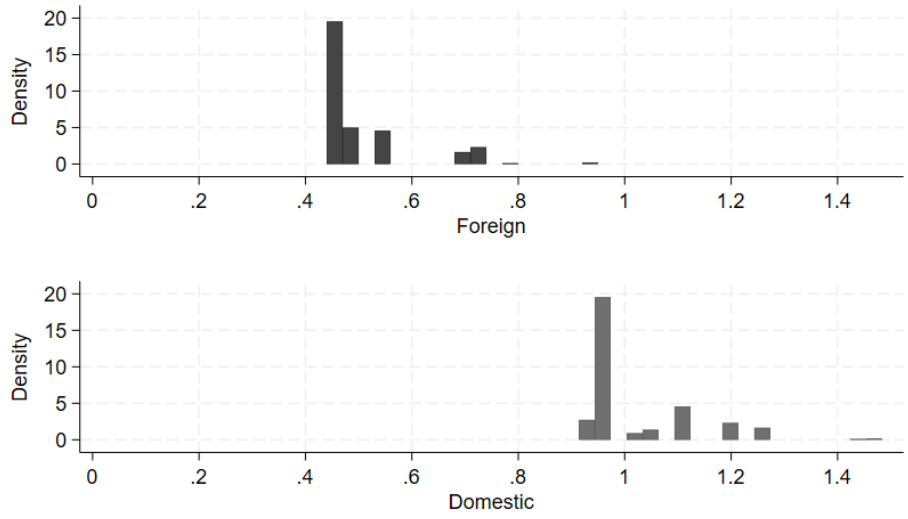


Figure 2: Distribution of minimum wage changes by district

3 Data & Set Up

3.1 Data

This study combines several datasets. First, we rely on the rich firm-level administrative data from Vietnamese Enterprise Survey (VES) to analyse the impact of minimum wage reform on firms' labour market outcomes in Vietnam. Second, to examine the effects of wage shocks in Vietnam on Japanese headquarters, we integrate various datasets from Japan to connect headquarters information with their subsidiaries in Vietnam. Finally, we conduct our original survey of Japanese MNEs to uncover the mechanisms through which they respond to wage increases in subsidiaries. This section describes the administrative data we use in our main analysis.

Vietnamese Enterprise Survey [VES] To analyse firms in Vietnam, including subsidiaries of MNEs, we extensively use data from the VES. The data is available through the General Statistics Office (GSO) of Vietnam. Each year, the GSO surveys all registered businesses in Vietnam. The dataset includes firm-level information such as 10-digit

tax ID (*mathue*), firms' geographical location, shareholdings by country, and key balance sheet items. Our variables of interest are namely total employment and total labour cost. We use the tax ID (*mathue*) as a firm identifier for the purpose of merging it with Japanese datasets[8].

Basic Survey on Overseas Business Activities (BSOBA) [KJ] The BSOBA is a firm-level survey of Japanese MNEs and their foreign subsidiaries. The survey is conducted annually by the Japanese government, and firms report overseas subsidiaries with at least 10% shareholdings. Second-tier subsidiaries are included if headquarters' shareholdings of a first-tier subsidiary as well as the first-tier subsidiary's shareholdings of a second-tier one exceeds 50%. The survey excludes the finance, insurance, and real estate sectors. The KJ dataset includes information on parent and subsidiary firms, such as their names, industrial sectors, years of establishment, and shareholding rates.

Toyo Keizai Overseas Japanese Companies Data [TK] The private company Toyo Keizai conducts survey on Japanese MNEs and their foreign subsidiaries every year. The dataset includes subsidiaries with at least 10% shareholding rates and indirectly invested firms where cumulative shareholdings exceed 10%. TK provides rich details, including names, addresses, industrial sectors, and shareholding rates of both parent firms and subsidiaries. While both KJ and TK provide invest information of Japanese MNEs, TK includes subsidiary address information, which is absent in KJ but crucial for merging with the VES.

[8] *mathue* is the only consistent variable across different years, making it suitable as a firm identifier in the panel dataset. Until 2015, the VES's raw data contained both *match_file_code* and *mathue*, while neither of them was unique. *match_file_code* facilitates merging diffrent survey modules and there were relatively few duplicates. Within a *match_file_code*, there may be multiple *mathue*. From 2016 onward, *mathue* becomes the only variable for a firm identifier, although duplicates still exist. To make *mathue* a unique identifier, we dropped observations where duplicates appeared within *mathue* in variables such as employee numbers, total compensation, turnover, and assets. These omitted observations represented less than 0.5% in the dataset. For remaining duplicates with *mathue*, we summed the values in the balance sheet items, assuming each observation represents establishment-level data. Each year, more than 99.5% of *mathue* observations are unique, with only a marginal share of firms requiring this operation of removing duplicates.

Basic Survey of Japanese Business Structure and Activities (BSJBSA) [KK] The BSJBSA (hereafter KK) is a firm-level survey of Japanese firms with more than 50 employees and capital exceeding 3 million Japanese yen (JPY). Conducted annually by the Japanese government, KK includes detailed information on firms' name, address, balance sheet items. KK can be merged with KJ via a shared firm ID. This dataset is primarily used to analyse the labour market outcomes at Japanese headquarters, such as employment and wages.

3.2 Constructing datasets for analysis

(1) Dataset in Vietnam The VES dataset, with *mathue* as the firm identifier, forms the basis of this analysis. Since the focus is on the minimum wage reform in 2011, the sample is restricted to the years 2006–2021. The years prior to 2011 are included to ensure the parallel trend assumptions. Our baseline year is 2010, and the raw data for 2010 contains information on 279,328 firms, of which 46,784 operate in the manufacturing sector. This study centres on manufacturing firms, comparing foreign enterprises with domestic private firms within this sector. State-owned enterprises, totalling 618, were excluded from the sample to ensure consistency with the focus on private and foreign entities. To only exploit firms that experienced the shock, we further dropped firms that stopped operation before 2010. The final sample consists of 41,958 manufacturing firms, of which 37,538 (89.5%) are domestic private firms and 4,420 (10.5%) are foreign firms. As of 2010, the largest foreign investors in the manufacturing sector were Taiwan, with 1,363 firms; South Korea, with 1,010 firms; and Japan, which ranked third with 593 firms. Figure A2 shows the share of foreign firms by ownership each year.

(2) Merged Japan-Vietnam dataset To analyse the connection between labour market variables in Japan and Vietnam, a parent-firm-specific dataset was constructed. Figure 3 shows the flow of the merging process. The goal is to link Japanese subsidiaries

in the VES with their parent firms in KK, enabling us to connect subsidiaries' wages and employment to those of their headquarters every year. To bridge these datasets, we use KJ and TK, which contain investment information about Japanese firms. Section A.1 in Appendix shows the detailed step of merges.

After merging, 1,431 subsidiaries in KJ (84.7% of 1,690) were matched with VES, of which 1,149 subsidiaries (68.0%) had parent firm information in KK. Between 2006 and 2021, 4,913 firms in VES reported Japanese shareholdings above 10%. Ultimately, 1,117 firms in VES were linked to parent firms in Japan[9].

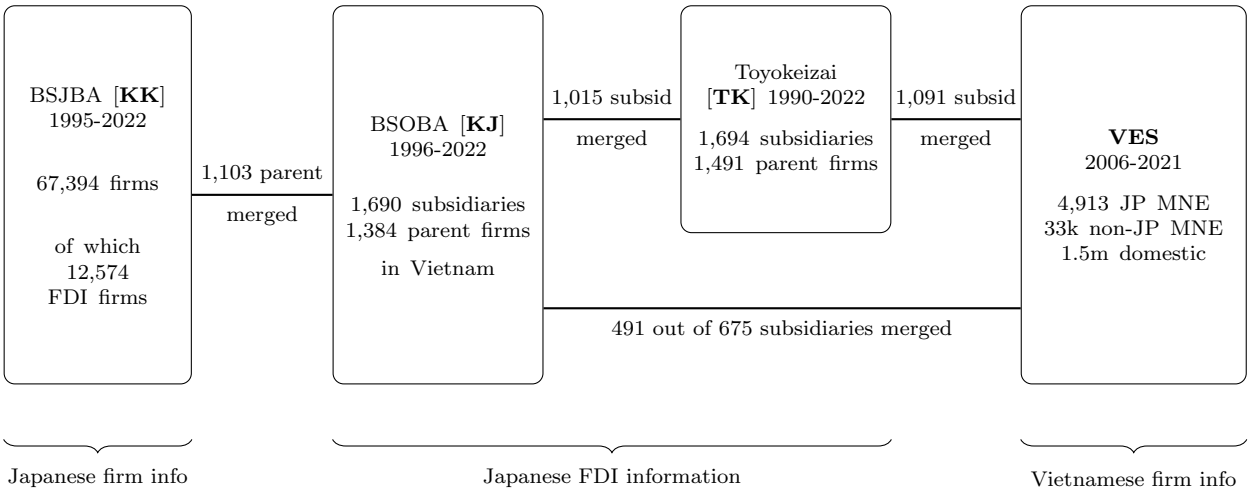


Figure 3: Merging process

We then converted the merged datsets unique at Japanese headquarter for each year, instead of subsidiary-unique. As a result, 1,038 Japanese firms in the KK dataset were successfully linked to employment and wage information for their subsidiaries in the VES. In the merged dataset, the average number of subsidiaries in Vietnam per Japanese firm is 1.31, with 864 firms (83.2%) having only one subsidiary. As 2010 serves as the

[9]There may be several explanations for the gap in the number of Japanese subsidiaries in KJ/TK and VES. First, while VES is a census, KJ and TK are data from survey, whose response rate is around 60-75%. Second, it is likely that the VES includes Japanese firms' overseas branch offices, while KJ and TK only have subsidiaries' information. According to Ministry of Foreign Affaires, in 2017, there are 1,177 Japanese subsidiaries, 323 overseas branches of headquarters, 59 firms with Japanese entrepreneurs. The number of Japanese subsidiaries in TK and KJ are 1,063 and 908, respectively, and this is closer to the official statistics.

base year, the analysis focuses on 920 firms that had investments in Vietnam during this period, of which 596 operate in the manufacturing sector.

(3) Survey of Japanese MNEs As an additional component of the analysis, we conducted a survey of Japanese MNEs in November 2024. We sampled 8,000 firms from Toyokeizai data for MNEs with current and previous investment history in Asian countries. The sample covers all 6,480 Japanese MNEs that hold subsidiaries in Asia in 2022, and we randomly selected 1,520 firms with past FDI history in Asia. In collaboration with a survey company, *Teikoku Databank*, we distributed a survey questionnaire to headquarters of sampled firms via mail posting, while allowing for responses both online and via mail. We received 1,388 valid responses (17.4%), of which 1,204 currently have subsidiaries in Asia, and 184 firms previously had subsidiaries in Asia. Although the survey itself does not specifically focus on Vietnam, 324 firms with subsidiaries in Vietnam are included (152 of them are in the manufacturing sector). Hjort et al. (forthcoming) will provide the full details of the survey and its results. In this paper, we present some of the results with a focus on their responses on wage increases in subsidiaries. This survey is meant to supplement our main analysis with administrative data.

4 Impact of Minimum Wage Reform in Vietnam

4.1 Pre- and post-reform wages

As discussed in section 2, the minimum wage reform in 2011 represented the largest shock in recent decades. While the employment is the main variable of interest, our interest also extends to the effect on wages. Figure 4 and Figure 5 illustrate the distribution of average wages for domestic and foreign firms (MNEs), respectively. It is clear that the

distribution shifts upwards for both groups[10]. Before the reform, foreign firms generally paid higher wages than domestic firms, and a larger proportion of domestic firms paid wages below the minimum wage standard that was set after the reform.

The shifts in wage distribution confirm that the minimum wage shock translated into a significant increase in wage levels for both groups. In particular, foreign firms paying by far higher than the new minimum wage standard suggests that the minimum wage shock not only affects low-wage firms but also firms in the upper tail of the distribution. Moreover, the difference in average and median wages between foreign and domestic firms become larger[11], implying that overall MNEs reacted stronger than domestic ones. This result is notable given the minimum wages shock was larger for domestic firms.

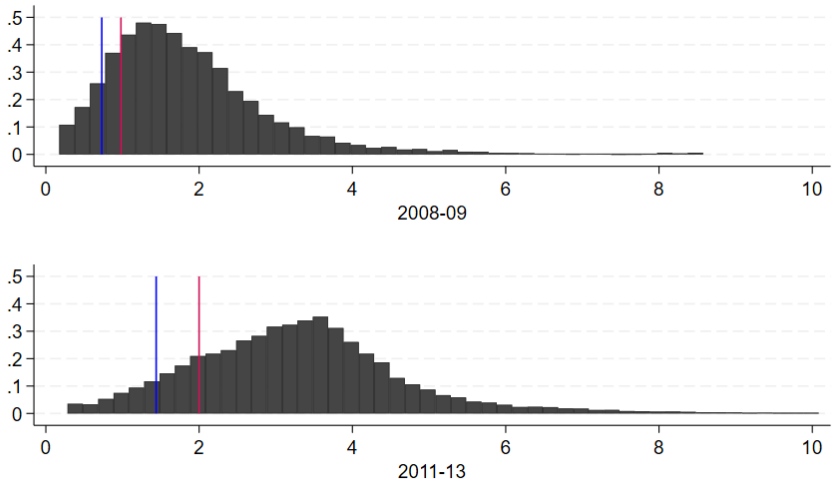


Figure 4: Distribution of average wage for domestic firms

Note: The figures show the distribution of firms by average of monthly wages (milllion VND) before and after the reform. Red line shows the highest minimum wage (Region I) and the blue line shows the lowest (Region IV) in each period.

[10]The figure shows a significant number of domestic firms still paying below the minimum wage levels after the reform. This may reflect the inclusion of part-time workers in the total employee count, which lowers the average monthly wage. Since the VES does not provide disaggregated wage data, we can only infer this from the total compensation values. Another potential reason is the non-compliance, as highlighted by Dube and Lindner (2024).

[11]The average wages prior to the reform was 2.97 million VND for foreign firms and 1.86 for domestic (difference of 1.01), which shifted to 5.33 and 3.32 million VND (difference of 2.01) after the reform. The median wages changed from 2.56 for foreign and 1.66 for domestic (difference of 0.90) to 4.77 for foreign and 3.24 for domestic (difference of 1.53).

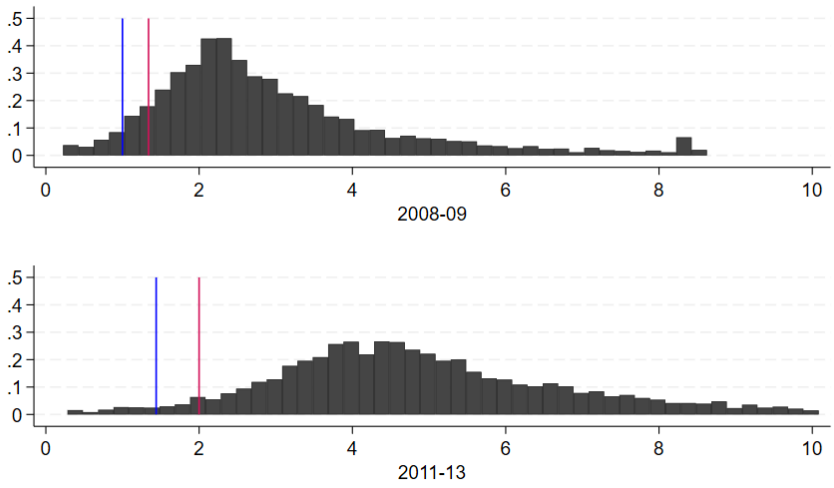


Figure 5: Distribution of average wage for foreign firms

NNote: The figures show the distribution of firms by average of monthly wages (milllion VND) before and after the reform. Red line shows the highest minimum wage (Region I) and the blue line shows the lowest (Region IV) in each period.

4.2 Estimation Method

To assess the significance of this policy change, we examine whether firms adjusted their employment following the reform. Specifically, we aim to estimate this effect by constructing a firm-level measure that captures the intensity of exposure to the policy change. We employ a firm-level event-study (DiD) approach with two-way fixed effects. The treatment and control groups are defined based on the average per-worker compensation prior to the reform in 2011. A key caveat is that the VES does not have individual-level wage data, so we cannot directly observe the fraction of workers whose wages were below the post-reform minimum wage level. For each firm, we define the treatment dummy as follows:

$$D_{id} = \mathbf{1}\{AvgWagePerWorker_i^{2008-2010} < MW_d^{2011}\} \tag{1}$$

where for firm i in district d, the dummy variable equals 1 if the average wage per worker between 2008 and 2010 was below the minimum wage level in district d in 2011. The post-reform minimum wage standard varies by firms' ownership being foreign or domestic. Table 1 presents the summary statistics for foreign and domestic firms, categorised by treated and control groups. Foreign firms have a relatively lower share of treated firms, while about a third of domestic firms fall into the treatment group. For both domestic and foreign firms, the control groups generally perform better on average, with higher assets and net turnover. Geographically, the proportion of firms in Ho Chi Minh City and Hanoi City is similar for foreign firms, while domestic firms in the control group are more concentrated in Ho Chi Minh City.

Although our interest would be to add another category of Japanese MNEs, the number of firms was not sufficient to conduct the analysis. There are only 64 treated firms among Japanese subsidiaries, making it difficult to estimate the effect specifically for Japanese firms. Thus, in this specification, we will only differentiate between domestic and foreign firms. We plan to extend the analysis using a district-level minimum wage exposure, for which Japanese firms may be separated from non-Japanese MNEs.

To compare the different effects on domestic and foreign firms, we employ a triple difference-in-differences approach, incorporating a dummy variable indicating whether a firm is domestic or foreign. Our explanatory variable is the triple interaction term of the binding dummy, ownership (foreign or domestic), and year. We estimate the effect of the

Table 1: Summary statistics of treated and control firms in December 2010

	Foreign		Domestic	
	Treated	Control	Treated	Control
Total employment	520.96	208.90	51.81	44.55
	(898.40)	(287.44)	(178.88)	(110.42)
Total compensation	522.29	617.29	66.43	125.78
	(689.44)	(612.73)	(192.82)	(279.68)
Average compensation	1.42	3.97	1.36	3.02
	(0.64)	(2.37)	(0.59)	(1.43)
Asset	73,193	91,137	12,050	19,658
	(113,828)	(117,460)	(39,409)	(53,362)
Net turnover	66,166	90,744	11,105	19,972
	(115,624)	(118,080)	(39,948)	(57,073)
Ho Chi Minh City (%)	19.18	19.28	13.54	40.97
	(39.41)	(39.45)	(34.21)	(49.18)
Hanoi City (%)	5.09	6.87	11.64	17.06
	(22.00)	(25.29)	(32.07)	(37.62)
N	617	3,803	10,314	27,224

Note: For each item the number above shows the mean and the number in the parenthesis is the standard deviation. Total compensation and average compensation are monthly values. Total compensation, average compensation, asset and net turnover is in million VND.

minimum wage reform using an event-study analysis with two-way fixed effects[12]:

$$\log y_{idot} = \sum_{k \neq 2010} \beta_k G_o \, D_{id} \times \mathbf{1}[t = k] + \alpha_i + \tau_t + \gamma_{ot} + \eta_{dt} + \epsilon_{idot} \tag{2}$$

where o denotes ownership and t is the year. y_{idot} is our variable of interest, specifically the number of employees. D_{id} is the binding dummy defined in equation 1, which is interacted with the ownership dummy G_o in pre-reform period. α_i and τ_t denote firm- and year-fixed effects, respectively, and γ_{ot} is the ownership-time fixed effects. Additionally, we introduce district-year fixed effects, η_{dt}, to control for district-specific time trends. η_{dt}

[12]Our empirical approach is the standard triple difference (DDD) method with multiple time horizon, and the shock is not staggered. In this case, the simple dynamic event-study TWFE approach is suitable and the heterogeneous treatment effects by cohort as in De Chaisemartin and D'Haultfœuille (2020); Sun and Abraham (2021) are not our concern(Roth et al., 2023). A similar identification is employed by Rao and Risch (2024) with firm-level exposure to the minimum wage increases. While our data is unbalanced, i.e. some firms may have missing entries in some years, the efficiency of the estimator is still preserved (Harmon, 2024)

is expected to control for minimum wage increases and regional category upgrades that occurred before or after the reform [13]. Standard errors are clustered at district level. Our base year is 2010, a year before the minimum wage reform. We include years 2006 to 2009 to pre-trend verification, and estimate the effect β for each year after 2011.

4.3 Results

Figure 6 shows the effect of minimum wage reform on employment. Both domestic and foreign firms show no significant changes in employment bewteen 2006 and 2009, suggesting there were no pre-existing trends. It is clear that only domestic firms were negatively affected by the reform, while foreign firms remained unaffected. The point estimate for domestic firms is a 13.6% reduction in employment in 2011, which further drops to 20.1% by 2016. The results further highlight the persistence of the impact, even ten years after the reform.

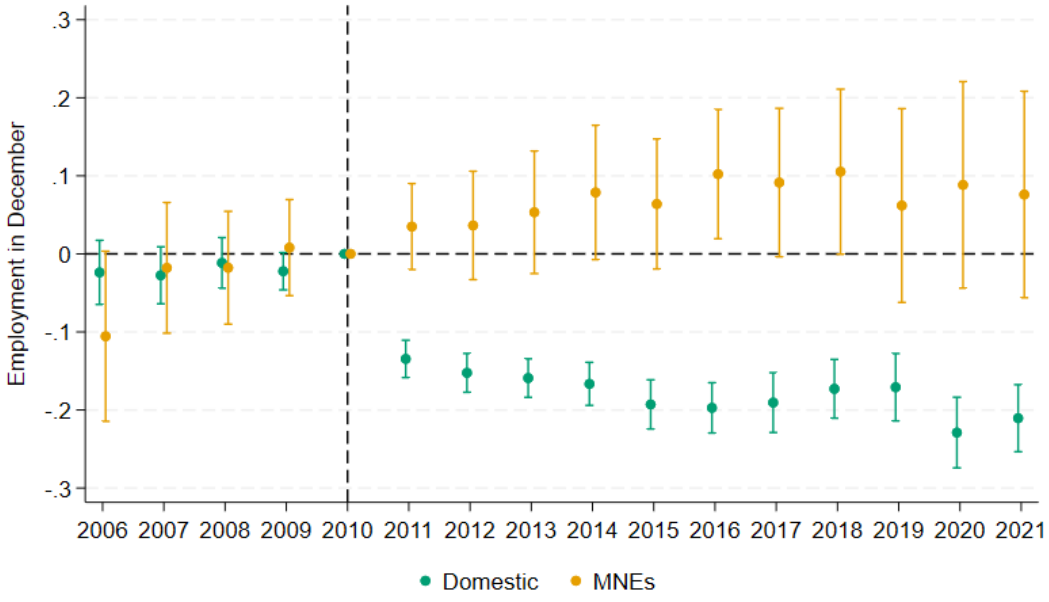


Figure 6: Effect on total employment

Overall, the results suggest the following story. Both domestic and foreign firms

[13]The impact of small-scale minimum wage changes is the future area of research.

reacted strongly to the 2011 minimum wage reform. Domestic firms mitigated the effect by reducing their workforce. In contrast, foreign firms did not adjust their employment levels and treated firms remained in the market. The result implies that foreign firms experienced a higher increase total compensation than domestic ones, even though the increase in minimum wages was lower for them than for domestic firms.

5 Impact on Headquarters in Japan

5.1 Estimation method

The previous section demonstrated that the minimum wage reform had a significant impact on labour market outcomes for multinational enterprises (MNEs) operating in Vietnam. A natural follow-up question is whether such changes affected the headquarters of these firms. This section explores whether the minimum wage reform in Vietnam had a meaningful impact on the parent firms, utilising the merged dataset described in section 3. Our sample consists of 562 Japanese manufacturing firms that invested in Vietnam in 2010.

The dataset is unique at headquarter-year level, and one firm can have multiple subsidiaries in Vietnam. To assess each parent firm's exposure to the wage changes in Vietnam, we sum the employment and total wages across their subsidiaries. For wages, we apply the yearly average exchange rates to convert Vietnamese Dong (VND) into Japanese yen (JPY) to capture the wage changes that the headquarters faced. We first employ the similar estimation method as equation 2 with the dependent variable being the total employment in Japan. Specifically, we estimate the following regression:

$$log\ y^J_{ipt} = \sum_{k \neq 2010} \beta_k D_{id} \times \mathbf{1}[t = k] + \alpha_i + \tau_t + \eta_{pt} + \epsilon_{ipt}$$

where D_{id} is the same as the binding variable defined in equation 1. For firms with

multiple subsidiaries, we defined the dummy as one when one of any subsidiaries are in binding. Thus, β_k estimates the treatment effect of Japanese firms that are required to respond to the minimum wage reform. y^J_{ipt} is the outcome of interest for firm i's headquarter in Japan in year t. α_i and τ_t are firm and year fixed effects, respectively. The subscript p indicates the prefecture in Japan, and η_{pt} captures prefecture-specific time trends. It is important to control for these prefecture-specific time trends because Japan experienced the Great East Japan Earthquake in 2011, which disproportionately affected the northern regions of the country. Including these fixed effects is crucial to minimize the impact of the earthquake and other local factors that might influence the results. In this specification, standard errors are clustered at headquarter level.

5.2 Results

Table 2 shows the summary statistics of Japanese headquarters by treatment status. Treated firms are on average larger in terms of employment in Japan and Vietnam, although the standard deviations are large due to the limited number of samples. Average compensation in headquarters are about the same level as well as the wage gap with their subsidiaries.

Despite the difference in the firms' characteristics, Figure 7 shows that the impact of the minimum wage reform on the Japanese headquarters is minimal. For all key outcomes, including total employment, total labour cost, and average labour cost per worker, the effects are insignificant from 2011 to 2017. We further analysed the effect on the share of labour cost as a proportion of total costs and the share of full-time employees, while these results also remain insignificant. Moreover, when we use minimum wage exposure instead of the increase in wages at the subsidiaries, the results are still null for all dependent variables.

Thus, the minimum wage reform in Vietnam did not have a significant impact

Table 2: Summary statistics of Japanese manufacturing firms in the merged dataset

	Treated	Control
Total employment in Japan	5808.9	2041.6
	(10468.8)	(4970.7)
Total employment in Vietnam	4487.5	440.5
	(3557.7)	(614.5)
Average compensation in Japan	4.814	4.870
	(1.622)	(1.587)
Average compensation in Vietnam	0.167	0.306
	(0.067)	(0.225)
Wage gap between Japan and Vietnam	4.646	4.689
	(1.601)	(1.616)
# subsidiaries in Vietnam	2.24	1.22
	(1.27)	(0.61)
N	25	537

Note: The statistics is for 2010. Average compensations are in million JPY for both Japan and Vietnam. Compensation in Vietnam were calculated by applying the annual exchange rate. Standard errors are in the parentheses.

on the parent companies in Japan. This is not surprising, given the persistence of large wage difference between Japan and Vietnam[14]. Even in 2017, labour costs in Vietnam accounted for only about 3 percent of the labour costs in Japan. Furthermore, the survey of Japanese MNEs confirms the finding. Among firms investing in Vietnam, more than 92.8% answered that an increase in wage levels in subsidiaries have no effect on wages and employment in Japanese headquarters. Thus, we find no evidence of labour substitution when MNEs experience wage increase from minimum wages in Vietnam.

[14]In our data, the annual average per worker labour cost in Vietnam was 0.30 million JPY in 2010, increasing to 0.74 million JPY in 2017, while the average labour cost at headquarters was 4.86 million JPY in 2010, rising to 5.16 million JPY in 2017. Furthermore, total labour costs in Vietnam amounted to an average of 171 million JPY in 2010 and 406 million JPY in 2017. In contrast, total labour costs in Japan remained relatively stable, at 12,475 million JPY in 2010 and 12,638 million JPY in 2017.

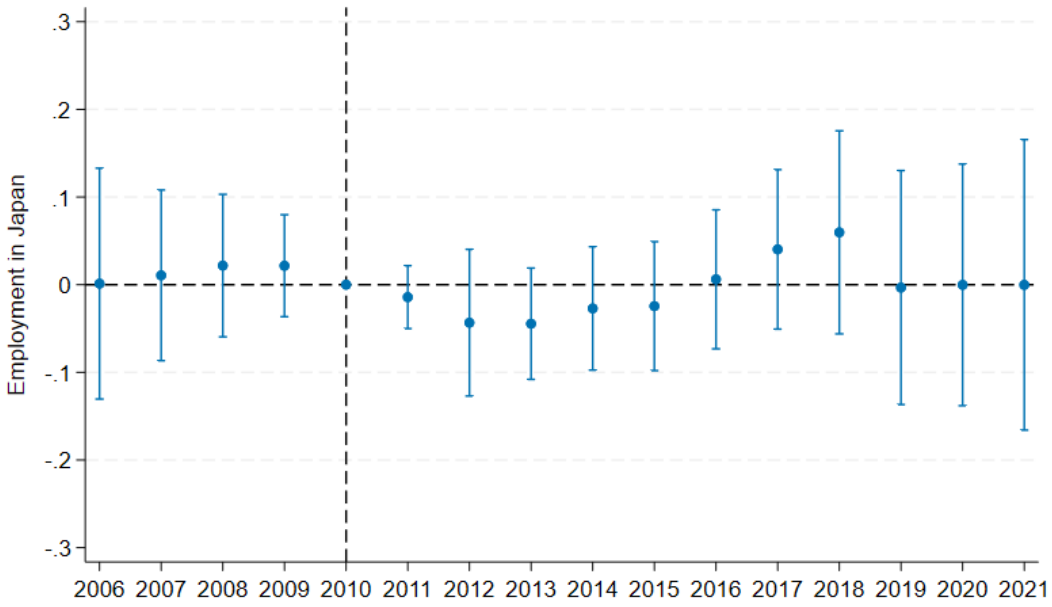


Figure 7: Effect on total employment in Japan

6 Conclusion

This paper has explored the impact of Vietnam's minimum wage reform in 2011. The government's intervention led to an average increase of 49.7% in the minimum wage for foreign firms and 102.7% for domestic firms. We focus on manufacturing sector firms to compare the effects on domestic private firms and foreign firms. Our analysis reveals that firms in both groups responded by increasing their labour costs. However, domestic firms mitigated the impact by reducing employment, whereas foreign firms did not adjust their workforce. This difference in employment responses led to a larger increase in total labour costs for multinational enterprises (MNEs), despite domestic firms facing a greater exposure to the minimum wage increase. This suggests that while foreign firms absorbed the increase in labour costs, domestic firms adjusted intensively.

We extended our analysis to study the impact of the minimum wage reform on Japanese headquarters, leveraging the merged dataset. While the reform led to a sub-

stantial increase in labour costs at the subsidiaries in Vietnam, it did not translate into significant changes in labour market outcomes at the Japanese headquarters. The effects on employment was insignificant, and we find no evidence of changes in labour substitution dynamics. These null results suggest that the wage gap between Japan and Vietnam remains substantial, allowing MNEs to absorb the increase in labour costs in the destination country without significant effects on the parent firm's operations.

Our findings suggest that foreign firms appear to passively absorb wage increases, while domestic firms adapted to the shock by adjusting on both the intensive and extensive margins. This finding may seem to contradict the prevailing literature, which often emphasises the strong influence of multinational enterprises (MNEs) on local labour markets. However, our analysis focuses on a relatively longer time horizon, and the context of the policy change suggests that MNEs were exposed to higher wage growth in the destination country compared to their home country. Thus, our findings contribute to the ongoing debate by showing that MNEs are not necessarily active drivers of local labour market dynamics, but rather, they are more likely to absorb the changes in labour costs imposed by the destination country.

There may be several reasons why foreign firms absorbed the increase in labour costs. First, foreign firms may have less flexibility in dismissing workers, as their labour force decisions could be partially determined by headquarters, and their compliance with labour laws may be stricter than that of domestic firms. Second, despite the significant increase in labour costs, the wage gap between Vietnam and the home country remains large, making continued operation in Vietnam the optimal choice. Headquarters may have factored in the higher wage growth in developing countries when deciding to invest, so that the post-reform minimum wage may have still fallen within an acceptable range. Exiting Vietnam and relocating to another country would be a costly decision, leading firms to bear the increase in costs. Third, MNEs may have benefited from the reform. Specifically,

the unification of minimum wages meant that foreign firms no longer had to pay higher wages than domestic competitors to attract high-quality employees. Such competition could explain why foreign firms did not adjust their employment levels. Finally, MNEs have advantage in responding to the risk when they invest in multiple countries. Even if a cost increases in one country, they can substitute their operation in another country without incurring a lot of cost. The analysis of these different mechanisms will be explored in future extensions of this paper.

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A Appendix

Table A1: Transition of Minimum Wages

Issued	In force		Region I	Region II	Region III	Region IV
Nov 2007	Jan 2008	Domestic	620,000	580,000	540,000	
		Foreign	1,000,000	900,000	800,000	
Oct 2008	Jan 2009	Domestic	800,000	740,000	690,000	650,000
		Foreign	1,200,000	1,080,000	950,000	920,000
Oct 2009	Jan 2010	Domestic	980,000	880,000	810,000	730,000
		Foreign	1,340,000	1,190,000	1,040,000	1,000,000
Oct 2010	Jan/Jul 2011	Domestic	1,350,000	1,200,000	1,050,000	830,000
		Foreign	1,550,000	1,350,000	1,170,000	1,100,000
Aug 2011	Oct 2011		2,000,000	1,780,000	1,550,000	1,440,000
Dec 2012	Jan 2013		2,350,000	2,100,000	1,800,000	1,650,000
Nov 2013	Dec 2013		2,700,000	2,400,000	2,100,000	1,900,000
Nov 2014	Jan 2015		3,100,000	2,750,000	2,400,000	2,150,000
Nov 2015	Jan 2016		3,500,000	3,100,000	2,700,000	2,400,000
Nov 2016	Jan 2017		3,750,000	3,320,000	2,900,000	2,580,000
Dec 2017	Jan 2018		3,980,000	3,530,000	3,090,000	2,760,000
Dec 2018	Jan 2019		4,180,000	3,710,000	3,250,000	2,920,000
Nov 2019	Jan 2020		4,420,000	3,920,000	3,430,000	3,070,000
Jun 2022	Jul 2022		4,680,000	4,160,000	3,640,000	3,250,000

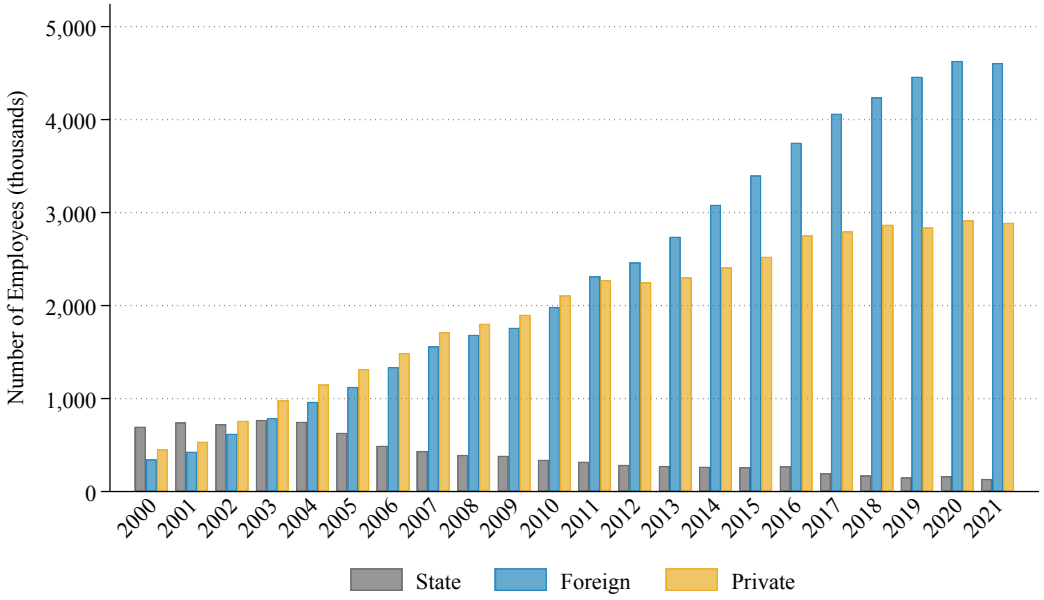


Figure A1: Number of employees by year

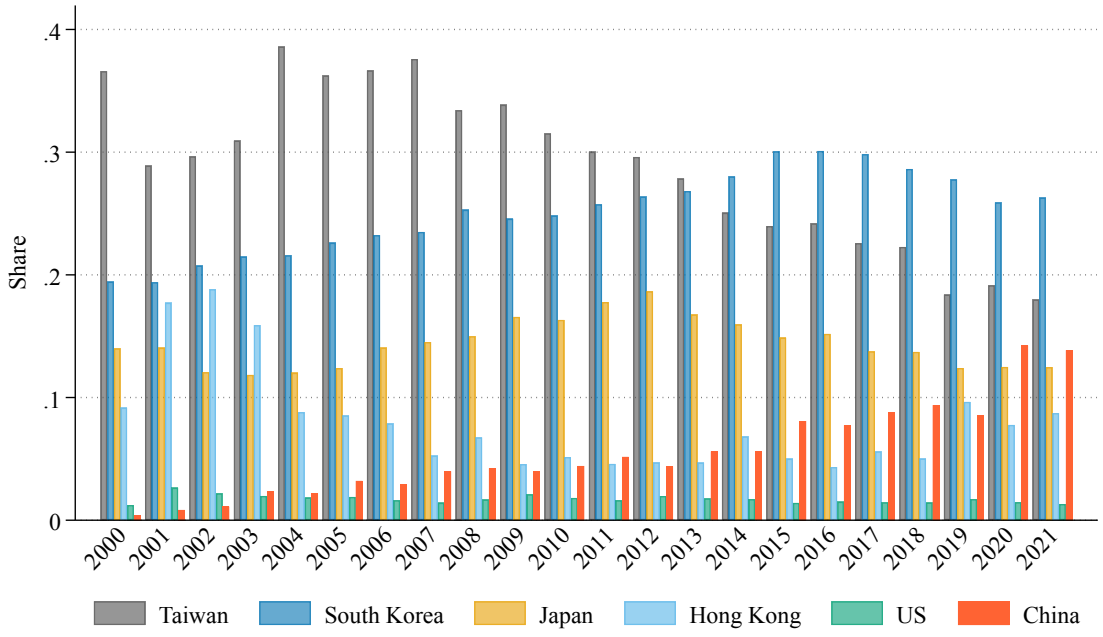


Figure A2: Share of MNEs by owner country

A.1 Details of data merging process

Merge of KK and KJ: KJ includes a firm ID from KK, allowing direct linkage. Since KK only surveys firms with more than 50 employees and capital exceeding 3 million JPY, small firms' subsidiaries cannot be linked. Of 1,384 parent firms investing in Vietnam listed in KJ, 1,103 (79.7%) are linked to KK. Consequently, 1,378 out of 1,690 subsidiaries in Vietnam registered in KJ (81.5%) are connected to parent firm information in KK.

Merge of KJ and TK: While KJ and TK datasets overlap, they differ in sampling criteria and response rates. TK provides richer subsidiary details, such as addresses, which is absent in KJ. We first merge KJ and TK to create a comprehensive dataset that links Vietnamese subsidiaries and Japanese headquarters. The initial merge process matches parent firms based on names and locations. Subsidiaries are then matched using variables like firm names, shareholding ratios, years of establishment, employee numbers, and turnovers. Of 1,690 subsidiaries in Vietnam listed in KJ (1996–2022) and 1,694 in TK (1990–2022), 1,015 subsidiaries are successfully matched.

Merge of TK and VES: Using subsidiary names in TK, we obtained tax IDs (*mathue*) in Vietnam via search engines[15], enabling linkage with the VES. In Vietnam, firm names, addresses, CEO names and phone numbers are the public information associated with *mathue*. We obtained *mathue* by manually searching for the firm names, and cross-verified with these information, which are also available in TK. This process linked 1,091 subsidiaries between TK and VES.

Merge of KJ and VES: For 675 subsidiaries in KJ not matched with TK, we directly linked them to VES. Using search engines, we retrieved tax IDs (*mathue*) based on firm names and matched them with VES. Since KJ lacks address and phone details, we verified matches using establishment years, employee numbers, and turnovers for cross-verification. This process successfully merged 491 subsidiaries (72.7%).

[15]There are namely three websites that we utilised: https://masothue.com/, https://doanhnghiepmoi.vn/, and https://infodoanhnghiep.com/